DOCUMENT RESUME

ED 054 696 FL 002 614

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TITLE Interdisciplinary Approaches to Language. CILT Reports and Papers 6.

INSTITUTION Centre for Information on Language Teaching, London (England).

PUB DATE Sep 71

NOTE 65p.; Papers read at a conference held in London, England, April 4-6, 1971

EDRS PRICE MF-$0.65 HC-$3.29

DESCRIPTORS *Child Language; *Conference Reports; Educational Disadvantagement; Failure Factors; Instructional Program Divisions; *Interdisciplinary Approach; *Language Development; Language Instruction; *Linguistic Competence; Linguistic Theory; Performance Factors; Psycholinguistics; Sociolinguistics; Speeches; Success Factors

ABSTRACT

The papers included in this publication were delivered at a conference initiated by the Centre for Information on Language Teaching in April 1971. Each of the four papers represents a distinct but related interest. The first, "The Language Barrier to Education" by B. Spolsky, examines sociological effects of differences between the language required in school and that used by pupils at home. By contrast, E.A. Peel's article, "Adolescent Concept Formation: Generalizing and Abstracting Processes," reports original research on the role of language in the growth of ability to generalize and abstract among adolescents during their education. D. Crystal defines the scope and value of stylistics as a branch of linguistic science in his "Stylistics, Fluency, and Language Teaching." The final paper, "Describing the Language Learner's Language," by S.P. Corder, defines what constitutes "error" in a learner's use of a second language. Commentaries on three of the papers are included. (RL)

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CILT Reports and Papers 6

Interdisciplinary Approaches to Language

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September, 1971
Centre for Information on Language Teaching

Published by the
Centre for Information on Language Teaching
State House
63 High Holborn
London WC1R 4TN

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Printed by Hanbury, Tomsett & Co. Ltd.,
Warner House, Warner Street, London EC1R 5ES.

Price 60p., post free in the United Kingdom.

CILT Reports and Papers 6

Interdisciplinary Approaches to Language

Papers read at a conference
held at Nutford House, Brown Street, London W1,
from 4th to 6th April, 1971

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Introduction

G. E. PERREN

Like any other fashionable word, *interdisciplinary* suffers a risk of being given extended and ill-defined meanings, even in academic usage. It may indicate little more than a passing collaboration; it may designate continued team work by various specialists; much more rarely, it describes research designed and directed by individuals fully trained and experienced in more than one discipline. Variation and imprecision in its use opens it to criticism :

> 'I feel that there is total intellectual confusion about the word "inter-disciplinary" in the universities . . . one discovers that something is classified as inter-disciplinary if it is about a topic that could be dealt with by different disciplines, or if the method of study, in, say, anthropological enquiry, involved some psychological techniques. I have not yet come across an inter-disciplinary study which is conceptually inter-disciplinary, and where the ideas are properly integrated between the various fields . . .'
> (Professor Marie Jahoda, *SSRC Newsletter 11,* March 1971.)

The use of techniques normally associated with one well-established discipline to further research in another does not itself make work inter-disciplinary. For example, the use of statistical analysis in agriculture or of computers in linguistics may well benefit agricultural or linguistic research, but tell us nothing new about mathematics or electronics. The techniques are tried, tested and available to assist work in any field where they may be appropriate.

While almost all the social sciences are to some extent concerned with language, linguistics has not (as yet) a ready-made set of techniques to offer the sociologist, the historian or the psychologist which are of general application. Linguistics may, however, even in its present complex state of growth and re-definition, provide insights at the same time as it elaborates its techniques. For if it exists to tell us more about language, the study of language may be necessary to tell us more about individuals and about society. Thus there may be areas of work in which the simultaneous development of techniques in two

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disciplines, co-ordinated and linked, can illuminate research in both. Psycholinguistics and sociolinguistics are examples where such alliances can be seen growing.

Traditionally many different disciplines have contributed to the general field of educational studies. Education, simply because it implements society's views about its own past, present and future, must pay attention to concepts arising from all disciplines not only in elaborating its current philosophy, but in arriving at practical decisions about what and how to teach at all levels, whether in the infant school or the universities. (One by-product of the multidisciplinary aspect of educational studies is, of course, that almost anyone assumes a right to express his views about the aims and content of the curriculum, whatever his intellectual home base may be.)

In recent years, concern with the rôle of language in education (whether it be the mother-tongue or a foreign language) has increased. Not only linguistics, but psychology, sociology and certain medical sciences now have a great deal to tell us about how language is acquired, how it may be learned, how it is used, and the part it plays in the development of human personality and social institutions. Such disciplines may even provide ideas about how language should be taught. Not that teachers should look to psychologists, sociologists and doctors for direct suggestions about classroom techniques; effective teaching methodologies result only from a great deal of filtering and refinement of theory in terms of pragmatic educational requirements.

Language is pervasive and hence there can be an essential linguistic factor in many academic disciplines, but the description and analysis of language for different purposes varies greatly. As it has often been remarked, language is itself a thoroughly unsuitable medium for discussing language — even among confessed linguists. But we have no other medium, and one of the problems of interdisciplinary work involving language is agreement about terms, or the establishment of a working metalanguage.

The papers which follow were delivered at an invited conference initiated by the former Committee on Research and Development in Modern Languages and convened on its behalf by the Centre for Information on Language Teaching, in April 1971. It brought together representatives of various disciplines, all of whom had a strong interest in language or linguistic techniques as a necessary component in research. Each paper is of importance in its own discipline and collectively they represent four distinct but related interests. That by Professor Spolsky examines certain linguistic dimensions of educational policy. Although his examples are largely drawn from American Indian schools, much of what he writes is relevant to any situation where there is a definable difference between the language required in school and that used by pupils at home. He uses linguistic insight to illuminate an educational problem. By contrast, Professor Peel's paper reports original research on the rôle of language in the growth of power to generalise and abstract among adolescents during their education. He writes from the viewpoint of a psychologist and educationist who is seeking to try out and refine new techniques of measurement which his subject demands — a difficult task since

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semantics is not one of the most tractable fields of modern linguistics. Dr. Crystal, as a declared linguist, seeks to define more clearly the scope and value of stylistics as a branch of linguistic science and to examine closely its relevance not only to the study of the mother-tongue, but to the teaching of a foreign language. Again he is exploring a hitherto ill-lit area. Professor Corder takes as his theme the potentially highly theoretical problem of defining what constitutes 'error' in a learner's use of a second language; as an applied linguist, he seeks to do so in terms which can be of maximum use to those who must decide on the methods and materials to be used for language teaching.

Three of these papers were also the subject of prepared commentaries, some of which contained different or complementary views. These have also been reproduced for they are of interest in their own right, as well as being found most useful as guides to subsequent debate.

— 1 —

The language barrier to education

BERNARD SPOLSKY

I

The general acceptance of the principle of equal educational opportunity, however it may be interpreted in practice, has forced educators with the need to explain why, in all societies, there exists a large group of children who fail to benefit from the schooling they are given.[1] The most convenient answer seems to be to blame the children themselves, arguing that there are one or more powerful factors that make them incapable of being taught.

One of the most powerful-seeming of these explanations is the genetic. Thus, when an American educational psychologist, Professor A. R. Jensen, interpreted some data he had gathered as establishing that 80% of intelligence was inherited,[2] his theory was quickly picked up. It was quoted in support of a wide range of positions calling for differential treatment of children as in Cyril Burt's paper in *Black Paper Two,* [3] or in the many and varied early childhood intervention programs in the United States.[4] This is not the place to summarize the extensive literature in which the inadequacies of Jensen's methods have been exposed and his thesis discredited,[5] but it is perhaps fitting to remark on one aspect of interdisciplinary research. There was a strong impact in Jensen's use of genetic, psychological, sociological, educational, and statistical data and techniques: while scholars qualified in each field noted the weakness of his case, they tended to assume it to be stronger in the other disciplines. When in doubt, I suggest as a motto for readers of interdisciplinary research, be doubtful of anything outside your field.

For those who wish to put the blame on environment rather than on heredity, there is the even more convincing explanation offered by those who have shown the close relation between intellectual development and fetal malnutrition. Studies of animals and humans have demonstrated the damaging effect of protein-caloric deficit before and after birth on the development of the brain. There is good reason to suspect, as Birch argues, that this interferes with the development of complex learning and behaviour skills.[6]

We are offered then some reasonable explanations of why children of lower socio-economic background do not do well in school, with better

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support for those who argue for the importance of early nutrition than of genetic factors. But a decision in favour of, or between, these two explanations is beyond our present scope, interdisciplinary though we may be. There is, however, another kind of popular explanation that is within our competence to judge, and that is the theory that poor children do badly in school because their language is deficient.

In its simplest form, the language deficit theory holds that many children are prevented from attaining full intellectual development because of their language. There are many formulations of the theory. The crudest are those that hold that only speakers of a given language (English? French? Italian? Navajo?) can ever develop the kind of perception necessary for science or logic or poetry or for distinguishing 225 basic categories in the cultural domain of objects at rest.[7] A more reasonable and extremely influential expression of the theory has been in the earlier articles of Basil Bernstein, who has argued that the lower working-class child's lack of experience with the elaborated code he postulates interferes with his power of abstraction :

> 'It is suggested that the typical, dominant speech mode of the middle-class is one where speech becomes an object of special perceptual activity and a "theoretical attitude" is developed towards the structural possibilities of sentence organization. This speech mode facilitates the verbal elaboration of subjective intent, sensitivity to the implications of separateness and difference, and points to the possibilities inherent in a complex conceptual hierarchy for the organization of experience. It is further suggested that this is not the case for members of the lower working-class. The latter are *limited* (Bernstein's italics) to a form of language use which, although allowing for a vast range of possibilities, provides a speech form which discourages the speaker from verbally elaborating subjective intent and progressively orients the user to descriptive, rather than abstract, concepts.'[8]

The language deficit theory may be offered as part of or as an alternative to the genetic and nutritional arguments. Mark Golden, for instance, reported on a longitudinal study of black children that led him to conclude that social differences in intellectual development were due neither to malnutrition nor heredity but to differences in the 'acquisition of abstract knowledge, the pattern of verbal interaction between parents and child, and differences in symbolic thinking ability.'[9]

There are a number of basic weaknesses with the theory. First, it is often based on inedequate observation, as Labov has shown.[10] The language used by a small black child in a formal interview with a large white educational psychologist is quite different from the language he will use in free interaction with another child. When the child's full powers of language are tapped, he turns out to master just that complexity of structure and richness of abstraction that the psychologist could not find, and the absence of which he adduced as evidence of deficiency. When the linguist studies the language of the lower class, he finds as full and rich a structure as in other language, just as so-called primitive languages have turned out to have the kind of richness of abstraction

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that I quoted for Navajo earlier.[11]

A second basic weakness is to assume that differences of language necessarily lead to differences of thought, or that language controls thought. The relation between language and cognition is sufficiently complex to provide ample work for all who wish to study it, but so far, it is only fair to remark that none of the empirical tests in which efforts have been made to establish the Whorf-Sapir hypothesis have shown other than trivial effects of language on perception and cognition.[12]

There is a more reasonable approach to the question: rather than emphasizing deficiency, what happens if we simply talk of difference? Can we still present an explanation of why some children do so badly? I believe we can. I will argue here that the language barrier to education is built, not by the child's deficient or inadequate language, but rather as a result of a number of erroneous decisions made by the school. In choosing the language policy that they do, schools often automatically discriminate against many of their pupils. Whether this should be the case is of course a political or philosophical question; that it is the case is observable if we apply to language education the tools of sociolinguistic analysis. And this is what I propose to do.

II

Just as language is central to human society, so the business of acquiring a language is a central part of socialization. In order to become a full-fledged member of his society, a child must learn his language, and the rules that govern how and when it must be used. One society with very specific rules is the educational system. My wife still wonders at the first thing they told her when she started kindergarten: if you want to talk, do it quietly. But for many children, the rules of speaking in school cause even more serious difficulties for they involve the use of a different language or dialect. This is a barrier to their education.

It is useful to distinguish between the informal and the formal acquisition of language and speech, using the term education to refer to the formal kind. A great deal of language learning is informal, left to unstructured exposure, the activities of such amateurs as parents and siblings, and the free working of exposure or unorganized reinforcement. This informal learning, it has been argued by Chomsky and his followers, is possible because of an innate language learning propensity, species-specific, that permits each normal human child to learn the language to which he is exposed.

There is good reason to believe that this informal language learning is structured too, but the structure is more from inside than outside the learner. Studies of language acquisition support the notion that all children learn, in about the same order, the main rules of language, although what this language is depends on their environment, and the speed and quality of acquisition, beyond a certain minimum, varies with individuals. Children, whatever their

cultural and language background, develop basic control of the sound system and grammar of their language by the time they are ready for school. This control, it appears, is a matter of forming an increasingly complex set of rules that slowly comes to approximate the rules of the adult speaker. By the age of thirty months, the child's sentences, which are still clearly governed by a child grammar, have up to five words. Understanding is much better than production. By the age of three, the child's vocabulary is about 1000 words, and most of his utterances can be understood even by strangers. But there is more to learn, and the next major task is development of style. The five year old child has a number of choices available for each utterance, and it is in the selection of the appropriate one that he continues to deviate from the adult norm.

By the time he is ready for formal education, then, the child has invested a great deal of time and effort in mastering language. He still has a great deal more to learn, especially new words and rules of how to use them, and even a number of basic grammatical rules.[13] But he clearly knows a language, can understand other speakers of it, and produce utterances that others understand, and all this has happened in informal interactions with parents, siblings and peers.

There are many cases in which the whole of an individual's language and speech acquisition is left to informal activities, but it is more often the case that a society makes some effort to formalize the education of its young, designating certain times and places for these activities, appointing certain people to be responsible for passing on certain areas of traditional knowledge and values, among which language and speech play an important role. These formal education systems may be described in a number of different ways : in this paper, my prime objective is to look at the sociolinguistic dimensions in order to see how the conflict between formal education and informal language acquisition arises. The first of these dimensions concerns the participants in the process : who is educated, and by whom (and for what linguistic role)? The second is concerned with the choice of linguistic channel : pre-literate oral, written, or post-electronic oral. And the third is the choice of code or language : how many languages (single- or dual- or multi-medium education), what types of language (vernacular, standard, or classical), and what registers?

Take first the participants. Who is educated? We should first distinguish between universal and select education. In universal education, it is agreed that all children in the society should receive about the same amount of basic education. What this amount should be varies : in modern times, most societies accept universal primary education as a minimum ideal; more advanced technological societies aim at universal secondary education, with various ideas of the amount and kind of secondary education; and some American educators speak of an ultimate right to universal tertiary education. Where there is selection, one of the more common criteria is sex, leading in most societies to a distinction between the amount and kind of education provided for males and females. Almost equally common is the notion of élite education, with those chosen being selected on the basis of descent, wealth, race, religion,

or intellectual qualification. Combinations of the two are also possible. One such model involves universal primary education, with access to secondary and tertiary education being dependent on descent, wealth, or intellectual qualification. This particular model ensures the continued enrichment of the governing group with the brightest members of the others.

The kinds and numbers of students turn out to be very important in the linguistic quality of education. As long as the students are selected from the status social group, there is likely to be homogeneity between the language of home and school, but as soon as school is opened up to all, there is an increase not just numerically but also in the number of languages and dialects of the pupils. There is good reason to believe that the failures attributed to modern education — such as its inability to teach Johnny to read — can in large measure be attributed to the rapid increase in the number of pupils, and the subsequent linguistic heterogeneity introduced into the school. A school system that chooses to admit pupils who speak many different languages or dialects, but treats them as though they all speak the same one is obviously discriminating against many of its pupils.

A second aspect of the participant dimension is the teacher. While teachers are often considered members of priesthoods, it is just as likely for them to be slaves, as in ancient Rome, or hard worked drudges, as the East European Jewish *melamed*. The modern status of the school teacher as a lower civil servant is an interesting compromise. In some societies, the teachers are élite guardians of an esoteric body of knowledge and a secret language. In modern society, however, with mass education, they tend to be members of the upwardly mobile lower middle class, one of whose self-appointed tasks is to teach the linguistic and other social mannerisms that must be acquired by those who wish to move upwards socially. These teachers may be native speakers of the pupil's language or of the school language, with consequent effect on the methods they choose and their effectiveness. The decision made by most modern societies to recruit primary school teachers from the very group that is most insecure about its own variety of language[14] produces a major strain. The case can be seen most clearly with vernacular-speakers who, having been forced to study in a standard language at training colleges, are the last to favor (or be able to assist in) education in the vernacular. Their tendency to choose as reference group the speakers of another language or variety has a serious effect on their attitude to language, and consequently on the attitude they pass on to their pupils.

In describing an education system linguistically, referring to the participants, we have the following possibilities :

The pupils may be linguistically heterogeneous or homogeneous, and the teachers may or may not speak the child's language. The first type of language barrier arises in this way : when the teacher does not speak the same language as the child. Before education can start, one must learn the language of the other. I am sure that most of us here today would have no trouble in documenting cases of this kind. Let me refer to one of which I have knowledge :[15] In a recent study we found evidence that over 73% of Navajo children come to school

not speaking enough English to do first grade work (we believe our figures to be reasonably reliable as a picture of the 50,000 or so Navajo children in school); but the chances these children have to be taught in Navajo is slight, for we estimate that no more than one of every twenty teachers in schools on the Reservation is a Navajo. It is not surprising to find serious educational deficiencies in Navajo children, nor to find that these deficiencies get worse the longer the child stays in school. The language barrier between teacher and child, created by a seemingly reasonable decision to use only teachers who meet regular certification requirements (a college degree and appropriate education courses), has proved too great for any methods or innovations to overcome.

Let us look next at the dimension of *channel*. In a pre-literate society, there is no choice : it must be oral. As McLuhan argued, the invention of writing, and, even more, of printing, changed the emphasis in education from aural to visual learning. In the oral system, one goes to school to memorize the traditional lore, whether learning it by heart, syllable by syllable, or learning how to recreate its essential content in appropriate formulas. When there is a writing system, however, the first task of the school is to teach reading and writing, and the written language becomes the special preserve of the school. Various reactions are possible. Orthodox Hinduism preserves the oral channel in its insistence on memorization of the vedas; orthodox Judaism, on the other hand, expects a reader who knows the Torah by heart to follow the place in the written text while reciting.

There are major differences between a written language and the spoken one of which it is a reflection that arise from the nature of the channel. Contrary to McLuhan's analysis, the spoken language is strictly linear : once an element is passed, it is lost. In writing, however, we can look over a sentence or a page again if we miss the meaning the first time. Consequently, the written language can have lower redundancy than the spoken, less repetition of words and ideas. Secondly, the writer has time to check over what he wrote while the speaker can only observe the reaction to his words. There is little problem in distinguishing between a radio talk being read from a script and one being given spontaneously. The written language is not the same as the spoken. To attempt to take a child directly from the one to the other has been recognized as creating unnecessary difficulty : it is usually realized that his first reading materials should be simplified; but it is still often not noted that even simplified reading materials are quite different from the child's spoken language.

For a long time, the emphasis on the value of the written language clouded any recognition of the nature of language as speech. There has been a new acceptance of the oral language, in these post-electronic times, where it is now possible to communicate over time and distance using speech : radio and telephones made recitations in class respectable again, and permitted the major swing back to oral second language instruction. But the continued emphasis on the written channel, and on the rules of the written language, forms the basis for a second language barrier to education. A classic example of this barrier is the language problem that marks the Arabic speaking world as a whole : the wide difference between Classical Arabic and the colloquial

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form of Arabic that is the vernacular of each area. This situation, labeled diglossia by Ferguson, itself creates major educational problems. In diglossia, two languages fill distinct roles in society. Classical Arabic is a written language with a long tradition, a closely guarded notion of grammatical and rhetorical correctness, and a network of academies and schools to preserve its purity. In each of the Arabic speaking countries, the vernacular is quite different : different phonologically, grammatically, and lexically both from neighbouring vernaculars and from Classical Arabic. The traditional view is that the vernaculars are corrupted versions of the classical language. Arabic speaking pupils coming to school

> 'have to unlearn or suppress most of their linguistic habits while trying to acquire new ones based on Classical Arabic as the language program requires.'[16]

The teachers themselves, it is to be noted, do not speak Classical Arabic in class, but continue to argue for it as an ideal. Two policies are proposed : a small group calls for the standardization of colloquial Arabic and its replacement of Classical; and the other, more influential, insist on the preservation, albeit with modernization, of Classical Arabic.

The final linguistic dimension of an educational system to be considered here is the choice of language or languages as medium of instruction and as school subjects, and the type of language each is. William Mackey has proposed a typology of bilingual education, parts of which are relevant here.[17] A school system can do its teaching in one language or more. A single medium school is bilingual if the language of instruction is not the same as that spoken by some of its pupils : to this extent, we can classify the schools on the Navajo Reservation as bilingual. A school may use two or more languages for instruction. The school might intend to transfer its students from the use of one language to the use of another, or to maintain them in the use of their first language. It may aim that the students master two or more languages, choosing that each language should be equal for all purposes, or choosing to make them different in status and use, with one language used in certain domains, and the other for other domains. The languages that enter into this picture may be of various types. Three of the classes set up by Stewart[18] are relevant to description of the educational system : vernacular, standard, and classical. For our purpose, we shall define these as follows. A vernacular language is the language or dialect spoken by the children at home; a standard language is the language or dialect used in the official, commercial and cultural life of the community; and a classical language is a language associated with national or religious history and traditions but that does not have living speakers.

With this last classification, we can consider the third kind of language barrier to education. The child acquires his vernacular in informal ways : the formal education system usually requires him to acquire either the standard language or a classical language. The real problem arises when it does not recognize the nature of the task.

The problem is, interestingly enough, less serious in the case of classical

languages. Classical languages are either taught to select élites, as was Greek in ancient Rome, Latin in Western Europe, or Sanskrit to Hindus, or taught with the full force of a flourishing religious system; as Hebrew in the East European ghetto, and Arabic in the non-Arabic Moslem world. And they are taught with the realization that learning a classical language is not simple; that it takes time and effort and that success can be achieved by few learners.

The teaching of the standard language, however, has been much less successful, for its complexity has often been ignored. Teachers and educational systems have often just assumed either that every child speaks the standard dialect or language, or that he will pick it up easily. Thus it has been and still is common for children to come to school and be classified as mentally retarded because they fail intelligence tests given in a language they don't know; to sit for years in a classroom where the teacher ignores that the whole of the class speaks a language he doesn't; to be treated as stupid because their dialect differs from the standard aimed at by the teacher. Even assuming the choice of an appropriate language or dialect, there remains the choice of register. Observers of classroom language have pointed out the prevalence of the two-thirds model; two-thirds of the time someone is talking, two-thirds of the time it's the teacher, two-thirds of the time he is telling children to do something. Those of us who have observed instruction in other than the child's language would add, and two-thirds of the time the children don't understand. The seriousness of this is emphasized by recent studies which show that first grade children do not usually understand the words most used in the teaching of reading. A particularly fine analysis of the problem is Douglas Barnes' study of the language of the secondary school classroom and its general inadequacy to promote interaction and learning.[19]

For learning is interaction : interaction with teacher, with book, or with peer, to mention only those interactions that must be mediated by language. School is not just a place that teaches language : most of its teaching takes place through language, and most of its learning depends on the pupils' ability to understand his teacher and the books used. Unless the teachers and pupils can communicate, there is little chance of effective education.

It is clear that some children bring many of their disadvantages from home : whatever their inherited capacity, their early nutrition can seriously damage their chances for development. But it is just as clear that the school itself, through its failures to recognize the complexity of the language problem, creates many more disadvantaged children. Inadequate and prejudiced views of the language spoken by the children provide excuses for lazy teachers, who explain their failures by talking about non-lingual children, or inferior vernaculars.

Whatever goals a society may set for its schools, they can only be achieved if the methods chosen are based on understanding of the child's original competence. It is quite indefensible to assume that children of lower socio-economic classes speak no language, or an inferior one, or an inaccurate form of the standard language. They have learned the language to which they have been exposed, and it is one with as much semantic richness and

structural complexity as any other. If the society believes that they must also acquire some other language, be it a different language or variety, then sound and effective methods of language instruction will be needed. But they cannot be developed from ignorance. Sociolinguistics provides some of the first knowledge that, added to what other disciplines can give us, will hopefully permit the development of approaches and techniques that will help overcome the language barrier that so many schools put in front of their pupils.

Notes and references

1. I wish to thank the Centre for Information on Language Teaching for the invitation to attend the Conference, and for the partial grant towards travel expenses. I wish further to acknowledge travel grants from the American Council of Learned Societies in conjunction with the National Endowment for the Humanities, and from the Association of Teachers of English as a Second Language, a section of the National Association for Foreign Student Affairs.

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11. The general position taken by linguists is 'that all natural language systems and "nonstandard" varieties thereof (with the possible exception of pidgins) are in fact equally capable of expressing and communicating thought of high degree of complexity.' Interim report of the Technical Committee on Language and Cognitive Development of the Linguistic Society of America, *LSA Bulletin,* 48 (February 1971) : 21-23.

12. From the same report : '. . . research has found only isolated and perhaps trivial instances in which the so-called Whorfian hypothesis of an influence of differential language structure on thought can be supported.'
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Commentary

G. E. PERREN

In his paper Spolsky is concerned primarily with language as a medium of education and not as a subject to be taught as part of the curriculum. His thesis applies essentially to the use of the mother-tongue in schools or to that of a second language wherever it may be found necessary to use one as a vehicular language for general subjects. In this respect it is of wide application. In some ways the place of English in schools in Anglophone Africa, or of French in those of Francophone Africa, may be comparable to that of English in relation to the Navajo, although there are considerable differences in the socio-linguistic factors; in Africa English or French is used by *African* teachers (to whom it is a second language) and not only by expatriates who themselves are ignorant of the children's mother-tongue. Moreover the use of English and French is fully acceptable to the pupils and to their parents as a desirable medium of education.

Much of his paper is highly relevant to education in Britain at a time when its structure and curriculum are rapidly changing and when traditional

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attitudes often seem inadequate to present day needs, complicated as these are by a considerable immigrant population upon whose satisfactory educational integration future social harmony may well depend.

Traditionally the values of Western European education have been intimately involved with language. The medieval trivium of grammar, rhetoric and logic — exercised originally through the learning and use of Latin — has had remarkable survival powers, although its progressive vernacularisation, generalisation, popularisation and vulgarisation have set our fathers and ourselves some difficult tasks of translation. Part of the educational value formerly ascribed to the study and learning of Latin has at various times been notionally transferred to the study and learning of modern foreign languages. Most recently, the practice if not the theory of education, has had to respond to the impact of mass media, which have deliberately adjusted traditional standards of expression and communication to conform to the psychological requirements of mass literacy at a comparatively low level of sophistication. The literacy formerly inculcated through the study of the classics, or of their institutionalised vernacular equivalents, at least developed powers of discrimination and selection. The wider and shallower literacy which results from today's universal education inevitably permits widespread vulnerability to intellectual exploitation, be it for political, commercial or moral ends.

Concurrent with the development of mass media, most of which to some extent involve the use of language, has been the explosive expansion of readily available (and assumed thereby to be useful) information. Where medieval education insisted on the value of philosophy, theology, logic or metaphysics as *intellectual systems* of ordering finite knowledge, and therefore proper to man's educational development, modern education, orientated towards scientific discovery and empirical knowledge, seeks above all to provide information. It may indeed be argued that the amount of information now considered essential in general education has outgrown the provision of moral and intellectual frameworks to contain it. Language remains the principal means of conveying information, although now there may be more talking (because it is faster) and proportionately less reading. More than ever, the process of education is linguistically saturated. Given the social heterogeneity of today's school population and the immense increase in the range and subject of the information to be conveyed, it is not surprising that the language channel sometimes appears to be unable to cope with the increased traffic it must bear. Efficiency of communication depends on the transmitter and the receiver as well on the efficiency of the link between them. It is small wonder that today's situation has led to a re-examination of the codes employed, especially in relation to those who must depend on schools as distinct from their home environment for the acquisition and development of verbal skill.

It may be claimed that the pre-eminence of language in western culture is being destroyed — for example by pop music and by computers.[1] Be this as it may, educational thinking now offers little alternative to the extended use

[1] George Steiner, *T. S. Eliot Memorial Lectures.*

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of language whether (in Spolsky's terms) it be vernacular, standard or classical. The apparent inability of many pupils to cope with the linguistic demands of the schools' curriculum has no doubt been instrumental in causing educationists, linguists and sociologists to elaborate certain notions and theories of linguistic deficiency. This may well be a kind of defence mechanism for educationists faced by a breakdown of communication in the classroom. As far as Britain is concerned, the 'Bernstein children' are only one aspect of the problem: immigrant children, both non-English speaking and dialect-speaking, are another.

Spolsky's suggestion then, that we should substitute the idea of linguistic difference for that of linguistic deficiency, has a force beyond that of just another educational euphemism. If it implies, as I think it must, that the school or individual teacher is wrong by not adjusting to the needs of the pupil rather than that the pupil is merely inadequate to the demands of society which are made on him through the school, it provides a positive approach to a distinctively modern theory of education — most necessary perhaps to those in Britain who have not yet fully adjusted to the implications of general comprehensive schools, or who accept almost unquestioningly a doctrine of democratised élitism.

Spolsky has distinguished between universal and selective education. We may ostensibly believe in universal education, even to the tertiary level, but is not education as it is organised even today inevitably self-selective? Is not selection *within the system* necessarily inherent *in the system* as it is now established?

Spolsky's notion of the teacher as a rather insecure priest, no less than as a socially conscious and occasionally rebellious slave, accords well with this situation and fits nicely with the concept of a civil servant. But perhaps this is not yet completely true in Britain, because of the survival of selection in its later garb of meritocracy.

'Teachers as élite guardians of an esoteric body of knowledge and a secret language' — this phrase appears to be particularly apt to the foreign language teacher in Britain at present, and the formal teaching of foreign languages, as distinct from teaching the mother-tongue or a vehicular language, is one of the concerns of this meeting. I feel that while we are searching for and perhaps finding an effective working theory of mother-tongue acquisition and development in schools, we are still a very long way from any adequate theory of foreign language learning in comprehensive educational terms. Clearly no foreign language can ever be learned in the same way as the mother-tongue was acquired, since learning the mother-tongue is necessarily a unique experience. Clearly no foreign language is as important educationally as the mother-tongue or vehicular language. Equally clearly there must be some educational reason for teaching foreign languages. I am not sure whether we are agreed as to what this should be.

We can of course fairly easily identify a vocational as distinct from an educational reason for learning foreign languages, but this will hardly apply

to, for example, primary school French or indeed to most secondary school foreign language instruction.

Most teachers in schools would like to be educators in the sense that they desire a wider educational justification for the content of what they seek to teach than mere vocational usefulness. In relation to the mother-tongue, the teacher's responsibilities, both educationally and vocationally, seem unlimited; but in relation to foreign languages it is difficult at present to see him very clearly as anything more than an instructor who remains vague about the educational purpose of his work, although he may be convinced that it has one.

In a final paragraph Spolsky notes: 'If the society (in which children are educated) believes that they must also acquire some other language (than their mother-tongue) then sound and effective methods of language instruction will be needed. But they cannot be developed from ignorance.' So far I believe that they have been developed, if not from ignorance then certainly from trial and error. And some of the errors have been considerable. In passing it may be observed that hitherto much of our empirical knowledge of how to teach foreign languages has been derived from experience with highly selected and thus more able children. The resulting methodology is therefore not likely to be appropriate to less able children, unless something was wrong with either our selection theory or our original methodology. In the mother-tongue we now seem to be in the position of believing that linguistic competence is inherent in every child, but being rather unhappy about its educational realisation or performance in many children. As far as teaching a foreign language is concerned, we may be trying to establish linguistic performance without any confirmation of a corresponding putative competence, beyond the support given by a theory of universals which needs much more exploration than it has hitherto had. Psycholinguistics no less than sociolinguistics should be concerned with establishing the valid theories of foreign language learning which educational philosophy sorely needs today.

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Adolescent concept formation

Generalising and abstracting processes

E. A. PEEL

1. The significance in productive language of the power to abstract
2. Ambiguity of the terms abstraction and generalisation
3. The absence of tests of abstractive processes
4. A distinction between abstracting and generalising
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1. *The significance in productive language of the power to abstract*

Although we recognize that a speaker or writer must be aware of the level of abstraction of the terms he is using, and that he makes this clear in his productive language to any audience, contemporary linguists do not appear to have studied abstraction processes in higher level language and thinking to any real extent. The study of the human knowledge expected of secondary school pupils and college and university students shows that as a student moves into adulthood we expect a growing capacity to comprehend and produce abstract verbal coinage. The abstract term is a very powerful symbol which enables the student to deal with large sectors of knowledge and thought in a precise manner. There is, of course, the danger that the abstract term also enables a speaker to slip over real understanding in a superficial way. But as Hayakawa[1] put it, consciousness of abstracting is a sign of adulthood. The significance of abstraction in ordinary language is brought out in the following statements :

(i) Apartheid is a denial of human rights.

(ii) *Speaker A* : 'John Gilpin' is my favourite ballad.
Speaker B : I don't think much of the ballad.

In the case of (ii) Speaker B is using abstract thinking in his reply, which in turn forces the first speaker himself to abstract in the subsequent discussion. B makes A concentrate on 'the ballad'.

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Yet another instance, taking us from the display of a piece of rock to the concept of metatheory, is illustrated in the following sequence :

(iii) This piece of green crystalline rock is malachite.
Malachite is an ore containing copper carbonate.
Copper carbonate is a chemical compound.
A chemical compound is a material substance.
A chemical bond is described in terms of the structure of electrons about atomic nuclei.
The notion of structure has been extended into the non-material disciplines, where it is known as structuralism.
Structuralism is an ultimate metatheory explaining a common mode of thought across the disciplines of mathematics, psychology, anthropology, linguistics and the like.

(iv) The ambiguity of the sentence : 'I hate fighting' depends partly on the abstract noun, denoting the idea of fighting.

Psychologists have given more attention to the problem of abstracting in mature thought, but earlier research and some based on behaviourist notions tended not to probe deeply enough. The logical insight provided later by Quine[2] has been remarked upon by a few psychologists (Braine[3]), but has not hitherto been translated into operational test material. One reason is that the process of concept formation involves abstraction and generalisation operating together, and it is difficult to tease them apart. My purpose in this paper is to do just this and to produce an operational technique for investigating the prevalence of abstracting thought during adolescence and early adulthood.

2. *Ambiguity of the terms abstraction and generalisation*

In studies of the child's ability to form hierarchies of classes, that is, more particular classes in classes having a greater coverage, we often find the act of inclusion described as abstraction, and at other times as generalisation. The two words are assumed to be equivalent in their meaning. In the very early studies of children's concept formation the process was indeed described as abstraction. Thus Welch and Davies,[4] writing in the now defunct 'Psyche' and setting out the plan of the researches Welch was later to carry out, headed their paper 'The Theory of Abstractions'. In this paper they used the instance of the logical hierarchy of concept : scarlet — red — colour, and it is clear from this paper and the later experimental researches by Welch that abstraction described a process which would now be more usually called generalisation. They divided 'abstractions' into two categories : undissociated and dissociated. The dissociated abstraction forms the subject of a proposition, while the undissociated abstraction is always the predicate of a proposition. They illustrate these ideas by referring to the attribute red :

The cherry is red — undissociated abstraction
redness — dissociated abstraction.

They go on :

'But far more important than this, the undissociated abstraction is

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genetically prior to the dissociated abstraction. The developing mind can grasp undissociated abstractions and fictional abstractional complexes of the first, second and perhaps even third remove before it can entertain any dissociated abstraction. The leap from the ability to entertain second and third remove undissociated abstraction or fictional abstractional complexes to the capacity for grasping the most elemental dissociated abstraction is one of the greatest in the history of a developing mind.'

However their use of attributes, as opposed to classes, obscures further real distinction, which is not between concepts which form either the subject or the predicate of a proposition, but *how* they may be used in the subject position. We may say 'Roosevelt was a man' but also 'a man is a living creature', where, in the first case man is undissociated, in the second, dissociated. But there is no difference in the logical status of these usages of the term 'man'. A difference comes about if we make the statement 'man is a biological notion'.

Welch seemed on the edge of making the significant distinction but in his later experimental papers[5, 6, 7] uses abstraction in the sense of extending the class, that is forming a superordinate class :

> 'By abstract thinking we mean the ability to include several or many smaller concepts in one large concept at the linguistic level. For example, the ability to think of this thing as a cat, that thing as a dog, another as a cow, and to think of all of them as comprising the class of animals, not sub-linguistically, but at a level at which we attach labels to all of these concepts.
>
> We are interested mainly in the linguistic type and in the various levels of abstract concepts which go to make up hierarchical structures. The class *"fruit"* includes apples, pears, etc., and the class *"vegetable"* includes potatoes, peas, etc. Fruit and vegetables thus are abstract concepts of the first order. They in turn may be species of a more inclusive class *"food"* which in this relationship would stand as a concept of the second order. The structures of our natural sciences often constitute hierarchies of as many as eight or nine levels.'

Similarly, two psychologists, Goldstein and Scheerer[8] used the term 'abstract attitude' to indicate how far a person is able to extend classes, as for instance being able to place, say, a pink skein of wool with either a red or light green one. Another test required the subjects to assemble common objects by their functions into classes of tools, cutting utensils, edibles and the like. In a real sense one can question whether any power of abstraction is called for at all in such tests. The distinction I am making is that put forward by Quine[2] where he distinguishes between the general term 'square' and the abstract term 'squareness'. Goldstein's patients might well have been able to put aside the squares and add others to them, but the test would not permit us to say whether the subjects had abstracted the concept of squareness. Quine goes on to say : 'use of the general term does not of itself commit us to the admission of a corresponding abstract entity into our ontology', but I am considering actual *language usage* where abstractions abound. My problem is the psychological one

of assessing the tendency to abstract.

Also in Hayakawa's writing[1] the idea of generalisation appears to pervade what he calls abstraction, as instanced in his 'abstraction ladder' :

Mrs. Plotz makes good potato pancakes.
Mrs. Plotz is a good cook.
Chicago women are good cooks.

The culinary art has reached a high state in America.

The first three rungs of the ladder demonstrate increasing degrees of generalisation, not rising levels of abstraction. Only in the last sentence has the level of abstraction increased in the term 'culinary art' and even here there may be some ambiguity, according to how we view the logical relation between 'a cook' and 'the act of cooking'.

Attempts to measure the 'abstractness' of productive language (Flesch[9] and Gillie[10]) reveal a similar confusion, since the count of different parts of speech and syntactic forms includes terms implying both generalisation and abstraction. They count what they call 'definite' words and then invert the final sum to produce their measure of 'abstractness'. The term definiteness is too vague for our purpose and this vagueness of course carries over into the inverse measure.

In general communication the word 'abstract' has several meanings. For instance it takes over some ambiguity from the two grammatical meanings of the word 'abstract'. First as an adjective in such expressions as 'this is too abstract' it may mean that the range of instances is too diverse for a concrete picture to be easy. A theory may also be considered very abstract if it is difficult to imagine a suggestive instance. As an adjective, 'abstract' is often used as nearly equivalent to 'general'. But when we say we abstract a rule we use the word in the intensive sense of finding the essential properties which define the concept. The equivalent meaning of abstraction is accepted for use here.

3. *The absence of tests of abstractive processes*

Many tests of verbal and spatial reasoning involve overt acts of classification and generalisation in one form or another. This is seen when subjects are asked to underline the odd one out, as in : apple, pear, plum, potato, peach, or to find the general term in : bus, car, vehicle, cart, lorry. Similarly in the sorting of pictures, blocks or objects, based on instructions to put together those which belong, success certainly hinges on the ability to classify and to generalise the rule of belongingness. Lastly the descriptive writing required of school pupils and involving sentences of the kind :

Henry VIII was a despot.
The French were victorious.
The peaks of northwest U.S.A. are volcanic in origin.
The steel workers came out on strike.

This implies the learner's capacity to include the particular subject of each sentence in the generalisation making up its predicate.

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But how far do such tests and sentences give any indications that the person can abstract? In the case of the verbal odd one out test the subject knows that he is dealing with fruit but the test does not allow us to find out if he is capable of the idea of fruitiness. In the case of sorting out, say, the red shapes from a collection of shapes of different colours, the successful child perceives which shapes are red but the test does not enable us to say whether he has abstracted the idea of redness. Similarly younger secondary school pupils may well know and be able to state that Henry VIII, along with other rulers, was a despot, but how many such pupils would be capable of beginning a sentence with the opening words : 'Despotism is . . .'?

The attempts by psycholinguists to assess a person's capacity to produce abstract language (Flesch op. cit.) show how difficult it is to prevent generalisation from intruding. For example the frequency of the definite article 'the' was counted as a measure of definiteness but its modifying function may indicate abstraction as in :

The space rocket is a twentieth century conception.

We need to consider it against the nouns it modifies. Similarly modification by the indefinite article or the absence of modification can be a marker of abstraction :

A community centre is important in town planning.
Location of worship is essential to all religions.

4. *A distinction between abstracting and generalising*

I shall refer to the concept of class, as implied in logical sentences of the kind :

Europe is a continent.
Lincoln was a President of the U.S.A.
'Ivanhoe' is a novel.
A novel is a piece of creative writing.

Here the terms in the subjects are less general than those in the predicates. Indeed three of the above subjects are particular, that is of zero degree of generality. But although the word 'novel' indicates a concept of higher generality than the specific written work 'Ivanhoe', it denotes a less general idea than a piece of creative writing.

But subject and predicate terms may also vary in level of abstraction. For instance we may compare the last of the above sentences with the following proposition :

The novel is a literary concept.

In this sentence we mean the idea of the novel, whereas in the earlier sentences we use 'novel' in a concrete albeit general sense. The idea of the novel may be said to be abstract, but it is also particular since we write of the idea of the novel.

My basic assumption then is that any object or concept term may be described in two independent dimensions : particular-general and concrete-

abstract. For any object or class therefore let us postulate one of

$g_o, g_1, g_{11}, \ldots$ degrees of generalisation, where g_o is the singular, unique, specific or particular, and

$a_o, a_1, a_2, \ldots$ levels of abstraction, where a_o is the concrete. In accordance with this description each object or concept will be described by a double index (a g). In the previous sentences, Europe, Lincoln, Ivanhoe are all $a_o g_o$ terms, whilst continent and President of the U.S.A. are $a_o g_1$ terms, and piece of creative writing is an $a_o g_{11}$ term. The change in status of the term novel from $a_o g_1$ in the third sentence to $a_1 g_o$ in the fourth sentence is crucial to this discussion, and I identify these changes,

$$a_o g_1 \longrightarrow a_1 g_o,$$

with the act of abstracting. Strictly it is the act of abstracting and particularising (or degeneralising), that is, it is the abstract-particular shift.

Here the shift is demonstrated in a sequence of five sentences changing both in generalisation and abstraction :

1. John Brown is a skilled worker.
2. A skilled worker is a productive individual.
3. Skilled worker is an important but numerically decreasing occupational category.
4. The occupational category is a socio-economic class.
5. The occupational category is a human concept capable of translation into operational terms.

In terms of the notation suggested we have :

1.	$a_o g_o$	is	$a_o g_1$
2.	$a_o g_1$	is	$a_o g_{11}$
3.	$a_1 g_o$	is	$a_1 g_1$
4.	$a_1 g_1$	is	$a_1 g_{11}$
5.	$a_2 g_o$	is	$a_2 g_1$

Large degrees of generalisation can be reached without recourse to abstraction, but high levels of abstraction can only be attained through the medium of increasing degrees of generalisation.

5. *The testing device*

In essence we have to test the shift of a concept from the concrete-general to the abstract-particular, in the notation suggested, from $a_o g_1$ to $a_1 g_o$. I propose to use verbal material to test the capacity to make this shift.

Often the change is revealed in language by the addition of suffixes to identify the abstract-particular. The addition of *-ness* and *-ity* to such attributes as *black* and *complex* give *blackness* and *complexity*. The change is shown in some class names by the addition of the suffixes *-ism, -ship, -kind,* as in the abstractions *cynicism, chairmanship, mankind.*

But there are many instances where the same word or phrase serves both $a_o g_1$ and $a_1 g_o$ purposes. Such a word is a homonym in a special covert sense since it is the common name for two forms of the same idea. Our problem

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is to engineer the setting of the material to produce an overt response. This can be done by leading the thinker from the concrete-particular to the concrete-general and then to call for a response at a third level. This third level is tested by a multiple-choice form, incorporating possible responses which individually involve generalisation, abstraction, constituent parts and partial associations. Here is a prototype instance. The subject is asked to underline the word or phrase in the column on the right which he thinks most appropriately completes the row of three terms.

Vesuvius (a_og_o)	volcano (a_og_1) (a_1g_o)	mountain (a_og_{11}) Italy (partial association) features of the earth's surface (a_og_{111}) geological notion (a_1g_1) lava (constituent part)

If the subject underlines mountain or feature of the earth's surface he is generalising further from volcano by respectively a smaller or greater degree. Hence I infer that the meaning of volcano is taken to be in the concrete-general form (a_og_1). If he underlines geological notion then I assume he is using volcano in its abstract sense (a_1g_o). The choice of Italy or lava would indicate restricted logical activity.

6. *Results*

A test of thirty such items as the above was produced, covering a wide range of content. This test was given to several groups of adolescents and adults, and the frequencies of Abstraction (A), Generalisation (G) and meaningful but alogical replies (O), counted and turned into percentages.

TABLE I

Frequencies of Response

GROUP	no.	Abstracting	Generalising	Others	Total
Newly graduated students	66	607	834	539	1980
Advanced Diploma students (non-graduate)	28	230	327	283	840
First year university students	22	149	275	236	660
First year college of education students	47	286	517	607	1410
5th year secondary school	70	389	712	999	2100
3rd year secondary school	66	221	623	1136	1980
1st year secondary school	63	138	561	1191	1890

These frequencies may be converted to percentages as follows :—

TABLE II

Percentage Responses

GROUP	Abstracting	Generalising	Others
Chance	20	20	60
Newly graduated students	30.7	42.1	27.2
Advanced Diploma students (non-graduate)	27.4	38.9	33.7
First year university students	22.6	41.7	35.7
First year college of education students	20.3	36.6	43.1
5th year secondary school	18.5	33.9	47.6
3rd year secondary school	11.1	31.5	57.4
1st year secondary school	7.3	29.7	63.0

From the above percentages it is seen that the capacity to make abstractions in the sense defined rises steadily through adolescence and into adulthood. The rise in power of generalising is less marked. The results appear to confirm that persons involved in higher level learning, such as carried out at the university, develop this capacity to use the abstract-particular form of concepts. Among the graduates, the English Honours students made a marginally larger number of abstracting responses. It should be noted that all the secondary school groups made fewer abstracting responses than would be expected by chance.

7. *Comments*

The above analysis of abstracting and generalising processes, and the device based on it for measuring the tendency to abstract, could open up several interesting studies on the thinking and use of language of groups of adolescents and young adults in different educational and cultural settings.

The scheme of analysis might well be applied to the investigation of the productive language of young people and at the least one could well use the existing device in correlational studies with English usage and other measures of abstraction. As regards the acquisition of English as a second language, it seems clear that one big difficulty is the use of the language as an abstracting device. All that has been said so far in this paper refers to English, but no doubt similar difficulties exist in the learning of foreign languages.

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Commentary

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The distinction between *generalizing* and *abstracting* has always been difficult to draw, and the need for it crops up in a variety of connections — non-verbal behaviour, artificial languages, particularly mathematics, natural language, logic and thinking. There are cases, perhaps chiefly occurring in the

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experimental study of animal behaviour, where within a particular context the distinction need not be drawn since nothing turns on it.

I shall do my best to start by taking up some points in Peel's paper *seriatim*. This may lead to a more essential statement of the problem as it appears to me in the particular connection of language and thought.

On page 21 Peel writes '. . . as a student moves into adulthood we expect a growing capacity to comprehend and produce abstract verbal coinage. The abstract term is a very powerful symbol which enables the student to deal with large sectors of knowledge and thought in a precise manner.' He goes on to mention '. . . the danger that the abstract term also enables a speaker to slip over real understanding in a superficial way', but does not seem to take this danger as seriously as I would personally be inclined to. The passage from the concrete to the abstract is, in language, all too easy, and linguistic devices for making it lie all too ready to hand. One such device is the coining of words, with terminations such as '-ism' or '-ation' and it is characteristic of fields of study, such as psychology and sociology, to introduce such terms in pursuit of their aspirations to resemble the more basically integrated sciences such as physics and mathematics. In so doing, it seems to me that far from *gaining* preciseness they *lose* it. The abstractness suggested in their vocabulary is too often absent in their thinking. Psychologically and logically abstraction involves far more than ascending to a contrived higher level of verbal or propositional form or meaning.

On page 23, introducing his view of abstraction, Peel writes 'a difference comes about if we make the statement *man is a biological notion*'. To make his point, he seems in general to have to appeal to a phrase — there are other examples on page 25 '*a novel is a piece of creative writing*' and page 27 '*geological notion*'. Does this mean that abstract terms are always verbally compound, often involving such words as *notion*? If so they need further definition, and few are agreed as to what such words mean. This difficulty can be, and often is, avoided by recourse to word-coining. In two of the above examples, for instance, we might substitute the coined words '*biologism*' and '*geologism*' — but this is pure verbal evasion.

To put abstraction, as opposed to generalization, on a basis other than a purely linguistic one, it is important to enquire whether it is to be found in non-verbal behaviour, and here I am doubtful whether Peel has sufficiently explored Goldstein's term 'abstract attitude' (page 23). It is true that in sorting skeins of different coloured wools into groups of hues, the patient might be said to be 'generalizing' or, as Goldstein would prefer it, 'categorizing'. But Goldstein contrasts the patient who can do this with the one who can only respond by direct matching of each skein to the next in a uni-dimensional series. The contrast Goldstein wishes to emphasize is that between *concrete* and *abstract* behaviour. The former is characterized by the patient being 'stimulus-bound', unable to detach himself from the situation as it presents itself to him, and to envisage it abstractly as being other than what it now is. The 'abstract attitude' is thus one of capacity for personal withdrawal from the actual situation and ability to regard it and deal with it in terms of its potent-

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ialities — that is in accordance with principles equally applicable to other situations. (It is not uninteresting to note that brain-injured patients whose abstract attitude is impaired are prone to emotional outbursts — 'catastrophic reactions' — on failure to perform allotted tasks.)

Again (page 24), Peel seems to regard all non-verbal tests — '. . . sorting of pictures, blocks or objects' as involving only generalization. There is, however, at least one such test in which I would think success depends upon the abstraction of a principle and its application in the choice of response. This is the Raven-Penrose Matrices Test, in which the subject must abstract one principle of change in the rows, and another in the columns, of a 2 x 2 or 3 x 3 matrix of diagrams, the last (4th or 9th) space being left blank for him to fill by appropriate choice from a number of presented alternatives. (It need not be emphasised that in the more complex cases where a 3 x 3 matrix is used, tasks involving extremely difficult feats of abstraction are involved.) It seems to me unwise to suppose that all that is going on here can be covered by Peel's definition of generalization.

As to *mathematics,* there must be many different senses in which the word 'abstract' has a definite meaning. Though pretty innocent in this field, I will suggest two examples. The first is that provided by the so-called 'Canonical Forms'. These are equations — to be found for instance in mathematical physics — which express the essence of other untidier and more materially-bound statements. Canonical Forms are characterized by the greatest possible simplicity, elegance, symmetry, freedom from numerical constants and independence of any particular co-ordinate system. The second example I will give displays both generalization and abstraction. This is the Hypergeometric Function F(a, b, c, x) one form of which is a power series. In this, appropriate choice of the constant terms a, b, and c and some simple transformations of the variable x gives rise immediately to the power series forms of the simple exponential, logarithmic and trigonometrical functions. In this connection the Hypergeometric Function may be regarded as a generalization of these. But in another sense the function is abstract inasmuch as it ties together a mass of otherwise unrelated, and more transcendental functions. I mention this case because it seems to suggest that abstraction is not necessarily definable in any simple way but requires reference to structures, and processes of thought, which lie outside the field of any particular items originally contemplated, which may themselves be susceptible to treatment in terms of generalization in the sense of class-enclosure.

In the field of psychology the terms generalization and abstraction have often been used as if they both pointed to the same process. There is still in psychology a bewildering variety of detailed theories of the mechanisms which underlie human behaviour, but I think it is fair to say that there is an increasing tendency to regard these as being hierarchically organized and in such a way that there is economy in the required storage of past experience. Such economy is not envisaged as mere subsumption of particularities into more general groupings but as abstraction of functional principles which, with greater or less adequacy, mirror the workings of the environment and of the

individual in his relationship to it. Some, notably the late Sir Frederic Bartlett, tried — though, so far as present views go, not very successfully — to characterize this form of organization in more detail, and adapted the term *schema,* as employed by the neurologist the late Sir Henry Head, to name it. Except in the form which Bartlett's ideas inspired the late Kenneth Craik to develop, Bartlett's particular theories find little favour today. Yet the general idea keeps on cropping up and it may help us to understand something of what is entailed by abstraction if we consider the views about conceptual thinking as suggested by A. W. P. Wolters, an early follower of Bartlett. Wolters attempts to state what *psychologically* as opposed to *logically* is characteristic of concepts. He remarks of the concept 'dog' (the term dog is both a superordinate and a concept) that so far as conscious awareness is concerned he can find nothing but (a) a dictionary definition of the word and (b) a general sense that 'I know what to do about dogs'. Wolters, therefore, proposes that it is the organzied system of reaction tendencies concerned with dogs — that is the 'dog-schema' — which constitutes the concept.

Perhaps we may now return to Peel's *volcano.* We may agree that this may be an abstract or a non-abstract term but I would suggest that psychologically, as opposed to what Peel would regard as *linguistically,* its abstractness consists in a complex, multidimensional and multidirectional set of reactive potentialities.

Thus we might give two examples of systems of internal reaction to the abstract aspect of the term *volcano* :

(I) Volcano⟶Geophysics⟶Mechanics of Fluid and Solid States in uncertain Equilibrium⟶Generalized (Abstract) Mechanical Theory;

(II) Volcano⟶Semi-stable Systems⟶Dynamic Demography of Animals and Men⟶General Ecology⟶Human Destiny.

We might compare these with an example which would *not* involve abstraction :

(III) Volcano → Occurrence of new volcano in Iceland between two visits I paid to that country →Changes in myself during that same interval.

One of the most satisfactory aspects of Peel's paper is his production of actual quantitative empirical data. His test could certainly cover the first two of the three examples I have just given together, doubtless, with many others. But what I have been concerned to suggest is that *linguistic* transitions do not necessarily correspond with anything that is *psychologically* or *logically* real and may equally well afford an appearance of doing so without any true foundation of correspondence with what, in Wittgenstein's definition of the World, 'is the case'.

Certainly young people at a certain age acquire the capacity (I would prefer the term 'tendency') to make abstraction. But I cannot help feeling that there is some tendency to induce in them this feat in a primarily linguistic

form — at least in certain educational disciplines. There are dangers in this and these apply equally to the teaching of foreign languages. What would seem to be needed here is the inculcation of an awareness of linguistic signals to forms of abstraction current in the culture of those whose mother tongue it is. These may be by no means obvious from mere linguistic comparisons between the two languages in question.

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Stylistics, fluency, and language teaching

DAVID CRYSTAL

Asserting the relevance of *anything* to language teaching is invariably a temerarious task, especially if one is a linguist, and not (except, perhaps, occasionally and mercenarily) a language teacher. But to make such a claim for stylistics, in the general sense in which I shall be using the term, seems to provide an absolutely safe and unimpeachable line for a temerarious linguist to take. The basic arguments are simple (indeed, to the unsympathetic, so obvious that it is surprising to see authors dwelling on them at such length), and have been frequently made over the past few years (as in Halliday, McIntosh & Strevens, 1964; Davies, 1968; or, more recently, Halls, 1970), so much so that it is nowadays almost a truism — at least, in the context of conferences of the present kind — to say that language teaching, whether mother-tongue or foreign, needs to recognise the fact of language variation within language, and to cope with it in some way. I shall therefore take it as axiomatic in this paper that language teachers, for various reasons (which I shall refer to below), wish to be aware of the range of systematic, situationally-distinctive variation in language, wish to make their students aware of it (at least as a theoretical terminus ad quem), and thus require techniques which will identify, classify, and, ultimately, explain the linguistic basis of this variation. Linguistics has undoubtedly been the main factor which has influenced the development of this situation; hence it is only natural that teachers, or teacher-trainers, who feel in need of information on these topics, will turn to linguistics for further assistance. And the point of the present paper is to ask frankly whether they will get it — or rather (to make my point of view clear from the very outset) to examine some of the theoretical reasons underlying why, in the present stae of the art, they will be unlikely to get it.

Before developing this point, a terminologal note is perhaps necessary. Stylistics for me is the linguistic study of systematic, situationally-distinctive, intra-language variation. By 'situation' I am referring to that sub-set of non-linguistic variables which a (linguistically untrained) native speaker can intuitively identify as accounting for a particular selection of linguistic features

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in a given (spoken or written) utterance.[1] 'Feature', in this definition, refers to *any* bit of speech or writing which may be singled out from language and discussed — a particular word, morpheme, sentence, structural relationship, etc. Now the above definition of stylistics means that my view of the subject is an extremely broad one — it subsumes both literary and dialectal use, for instance — and a word of explanation for this breadth of definition may be useful at this point. It seems to me that stylistics cannot be meaningfully restricted to the study of literary texts only, as the linguistic explication of such texts is theoretically dependent on the prior explication of non-literary variation (I am not of course suggesting this as a necessary *pedagogical* procedure). To recapitulate the argument I have used elsewhere (see Crystal & Davy, 1969 : 79,ff) : literature is in principle mimetic of the totality of human experience — by which I mean that there is no subject-matter or mode of linguistic expression which is a priori incapable of being introduced into a work which, by critical consensus, will be considered literary. But the phrase 'the totality of human experience' comprehends linguistic experience, as well as all else; and consequently we have to argue that the identity of literary expression is, in large part, definable only by relating it to the range of linguistic forms available in the community as a whole, which the writer has, consciously or otherwise, drawn upon. In *The Waste Land,* for instance, we find lines reflecting conversational, legal, religious, scientific, archaic, and other kinds of English, as well as bits of other languages. Clearly, in order to appreciate anything of the purpose of this combination of effects, we have first of all to recognise their presence in the text, and this in turn reduces to a question of the extent of our previous linguistic experience, and our conscious awareness of it. Another example would be the way in which stylisticians relate their observations about linguistic originality (or deviance) in literature to 'ordinary' language, in some sense (cf. Leech, 1969, and references there). As a result, I think it is essential to argue for a definition of stylistics which subsumes *all* systematic variation within a language accountable for by postulating that its occurrence is restricted (in some probabilistic sense) to norms of behaviour characterising social groups or (secondarily) individuals.

The question of what 'upper bound' to give the domain of a stylistic theory — in other words, how widely does the notion of 'social group' extend? — is not in my view answerable at the present time, and I do not propose to take up a position on this issue here. One might, for instance, decide on a fairly restricted definition, seeing stylistics as the study of the range of situationally conditioned *choices* available to native speakers, and of the varieties which sets of these choices constitute, thus excluding such variation as is studied under

[1] I thus wish to distinguish this from the more general sense of 'situation' used in some approaches to language teaching, where it is argued that new linguistic forms should always be presented with a distinct 'situational context', which makes the form meaningful to the learner (cf. Wilkins, 1971). Only a sub-set of situational constraints are referred to in my use of the term — namely, those which account for the selection of one rather than another of a set of well-formed linguistic alternatives. For further discussion, see Richterich & Marchl (1970). A detailed analysis of the variables referred to in my definition is to be found in Crystal & Davy, 1969.

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the heading of dialectology (either regional, social, or historical) on the grounds that choice, in any meaningful sense, is rarely a relevant factor in the linguistic analysis of these situations. Dialect features are background features, in this view, unaltering and unalterable features of a person's sociolinguistic identity, *against* which stylistic features can be seen to operate. On the other hand, one might decide to play down the criterial status of choice as being too unverifiable and too speaker-orientated, and concentrate instead on a holistic approach to the analysis of an utterance's extra-linguistic information, seeing dialect features alongside other features of social status, occupation, and the like, in an integrated model, all contributing to a speaker's sociolinguistic 'profile'. Which of these approaches (or any other) is likely to produce good results is not demonstrable until such time as a vast amount more data has been accumulated illustrating the nature of the supposed situationally-conditioned linguistic distinctiveness. On this topic, we are, very much, thinking in the dark : we are trying to solve a theoretical issue without having any clear idea as to the nature or extent of the problem in the primary data which the theory is supposed to be accounting for. For historical and methodological reasons, three 'branches' of study have developed — stylistics, sociolinguistics, and dialectology (this list could of course be extended). But the existence of these branches does not mean to say that the data, when we have analysed it, will best be accounted for in terms of a model which recognises these distinctions. We shall have to see. And meanwhile, it seems useless to go into questions of boundary-definition : it is certainly a red herring to raise this issue as a problem when one is trying to relate stylistics to a field such as language teaching. Any stylistic model is inevitably going to be to some extent arbitrary at present; and practical considerations are going to be primary in any questions of evaluation.

What the previous paragraph amounts to is the assertion that in this field, as in so many others in contemporary linguistics, theorizing has gone far ahead of experimental evidence, and as a result, pseudo-procedures and pseudo-problems have multiplied. The problem is not simply that few experiments have been carried out; rather, there have been few hypotheses formulated in ways which are testable — and indeed, a concern to think in terms of rigorous hypothesis-testing at all is sadly lacking in the published literature. But sophisticated speculation, no matter how stimulating, is not science. And surely this is the point. If the linguist is supposed to be claiming that his approach to stylistic variation is valuable, *because of its scientific basis,* then he must live according to his beliefs, and work in a scientific way. I know lip-service is paid to many a scientific notion in this connection : we frequently hear talk of stylistics being, or needing to be, 'objective', 'systematic', and 'explicit' — I have said this myself on many occasions. Moreover, I believe that such scientificness is indeed possible, and will be beneficial in the way in which present-day linguists claim. But I have come to believe that we are fooling ourselves if we think that what passes for stylistics at the moment is scientific in any genuine sense. Because we have had a few successes in analysis, and a generally favourable reaction from the language-teaching world, this does not constitute a validation of any theory or method. There are many brilliant stylistic analysts who are not linguists, many teachers who did successful

coursework on registers before the word was ever invented — as well as many perceptive linguists who transcend the limitations of their own methodologies. We have to be *sure* that it is linguistic stylistics which is improving things; and at the moment I don't see how we can be, as precious little self-criticism and real experimentation have taken place.

Of the three criteria of scientific thinking mentioned above, I think that current stylistic practice would get good marks for systematicness, but would probably fail in objectivity and explicitness (a full discussion of these terms is to be found in Crystal, 1971a). Let me try to substantiate this point with reference to objectivity. Emphasis on the need for objectivity in stylistics is so general as not to require quotation. It arose largely as a direct reaction against the impressionism and use of unverifiable value judgement which characterised so much of the talk about (especially literary) style. Stylistic statements were to be descriptive, not evaluative; they were to be substantiatable by reference to quantitative reasoning; they were to be phrased using a terminology which would be generally applicable; and so on. Largely as a result of this, the role of the subjective in stylistic research came to be minimised, and it has often been ignored. This was an unfortunate development, in my view, as it has fostered a conception of stylistics as being more objective, and hence more scientific, than it really is. The reason for this is that there are at least three places in any stylistic analysis where reliance on qualitative criteria of some kind is unavoidable: in the selection of data for analysis, in the analyst's identification of contrasts, and in the assessment of overall stylistic effects. Ignoring the problems posed by these areas can have serious consequences for the subject, as we shall see. I shall look at each of these topics in turn.

The standard research strategy in stylistics is to take some texts (I use this term to refer to either spoken or written discourse) and examine them to see if diagnostic features can be identified. But where does the researcher get his texts from? What criteria is he bearing in mind when he decides which texts to select? If he decides to investigate, say, the language of science, then this assumes he has some kind of intuition that there are features of language which correlate in some predictable way with certain events in non-linguistic behaviour ('situation'), which are generally and cumulatively labelled as 'scientific'. But who provides the initial assessment of the situation which allows him to select some linguistic material as being representative of scientific behaviour? How does he know, in advance, that his texts are valid samples of data, *relevant* to his hypothesis? His own intuition cannot tell him, as clarifying his intuitions about his data is the whole point of the exercise. And he cannot just assume that his sample is valid. For what does 'valid' mean here? At the very least, it seems to me, it presupposes the notions 'successful', 'satisfactory', or 'accepted'. It would be of little value a stylistician taking as a sample text a book which scientists generally recognise as being badly written, unscholarly, ambiguous, and the like. The possibility of obtaining an inedaquate sample has got to be eliminated, and this inevitably involves obtaining some kind of qualitative reaction from a native speaker of the language being studied (in this case, a scientist). But I am not aware of this having been done systemati-

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cally, or being considered as a routine check in research strategy.

It is perhaps not so obviously a problem in the language of science, where criteria are often quite explicit (as in the *Handbook for Chemical Society Authors*), but consider the difficulties we are faced with in evaluating the basis of a sample for such hypothetical varieties as advertising, journalese, political speaking, or sermons. How do you assess, or even obtain information about, the 'success' of an ad? One would not want a research student to use as his primary data a set of advertisements which an agency had criticised as poor, or which the public had failed to react to in the desired way. It would follow, then, that for any research in this field to be valid, one would at the very beginning have to do some market research into market research — to understand what the advertiser is trying to do, how *he* evaluates his material, and its effect, and so on. But if the researcher does so, he immediately finds himself faced with a highly subjective, intuitive area, which he will have to assess in its own terms, before he can introduce any kind of 'objective' reasoning into the exercise. Now as far as I know, this kind of 'contextualisation' is not a routine part of stylistic investigation; and to the extent that one thereby ignores causative factors affecting the nature of one's data, and fails to control them, one's hypothesis thus becomes non-rigorous, and one's results uninterpretable.

The difficulty, of course, increases along with the diminishing 'concreteness' of the variety being investigated. Advertising is a fairly well-defined field, with fairly explicit techniques and well-understood purposes; the important variables are relatively easy to isolate and define. But if we take a sermon as our object of study, the techniques, purposes, significant variables, and so on, are much more difficult to pin down. I do not think it would be too difficult a task to work out a questionnaire in order to establish the 'success rating' of advertisements, but my mind boggles at the way in which a sermon might be comparably evaluated. Can one stop the congregation as it leaves, and ask? Or should one work behaviourally, and quantify the intensity of the silence during it (a pin-dropping measure, for instance)? These problems are real, and they become dominating in cases of literary analysis. For example, if a student wishes to do some work on Dylan Thomas's poems (as seems usual), then he will generally make a selection to begin with — and initially, obviously, he will have to start with a given one. But which? May his choice be random? I do not think it should be; nor, indeed, do I think it can be (but this is a side-issue). Whichever text is analysed first is inevitably going to establish certain preconceptions about the subsequent analysis, some of which may be quite misleading, as far as ending up with statements of typicality are concerned. A great deal of harm has already been done to Thomas (and to poetic analysis in general) by students who have investigated his language in the firm belief that most of it was going to involve stylistic effects like 'a grief ago'! Not only has the collocational issue been rather overdone, as a result, but other, equally important features of Thomas's style in phonology and syntax have been ignored. To minimise the possibility of making his sample atypical, then, a researcher should try to make some criteria for selection explicit; and my point is that this rationalisation is always going to be evaluative. Either he will rely on his own personal feelings towards the poetry, or (as I recommend my students

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to do) he will rely on the impressions of the next best thing to native speakers of Thomas's poetry that exist, namely those literary critics who have made specialist studies of Thomas. It would be a rash stylistician who chose to work on Thomas using a text which was generally agreed by Thomas critics to be sub-standard. (He may of course decide to research into precisely that issue. Why is it a bad poem?, but this is a different matter).

The scientific course in such questions, it seems to me, is not to work at our analyses as if the problem did not exist, or to think it trivial, or perhaps to assume that its solution is someone else's province, but rather to face up to the necessity of devising techniques for coping with evaluative criteria and relating these to our own, more familiar, linguistic ones. And such techniques do not exist. Which means that here is a point of weakness in stylistic research strategy, that anyone wishing to make use of the strategy should be fully aware of.

Moving on now to the second place at which evaluative criteria are inevitably introduced into our stylistic investigation, we can establish a similar weakness. When we have actually chosen a text, and got it in front of us, then how do we go about establishing 'objectively' the relevant stylistic effects? Once again, the procedure which seems generally in use is quite un-objective. To begin with, there seems to be some reliance on an assumption that is regularly false — that stylistic effects in a text stand out clearly. This is certainly a feeling that many students have. It is probably our fault, a product of the general and natural tendency in published discussions on stylistics to make use of the clearest possible examples as illustrations of general categories. I am not of course denying the existence of some clear, unambiguous cases of stylistic effect, e.g. the 'thou knowest' kind of feature, which is predictably religious; but I am beginning to suspect that such effects are not in the majority, in a language. Once we have worked through the obvious varieties, like science, religion, law and so on, then we come to a vast no-man's-land of usage, where there are clear lines of situational demarcation, but few readily demonstrable stylistic markers. After Davies's (1968) references to the register of 'policemen's English', I have heard people talking about 'traffic-wardens' English', and worrying because they could not find clear distinguishing features, apart from subject-matter. (I am always intrigued as to how these people get their data!) It does not seem to have occurred to them that perhaps there are no distinguishing features to be found. Many stylisticians seem to have assumed that because language displays situationally-distinctive variation sometimes, therefore it always does, on any occasion when it is used. Now, as a working hypothesis, to focus attention and get some research moving, there is some point in this; but now that some examination of data *has* taken place, we must surely begin to realise that it may not always be so — or, if this is too strong, that it is not always going to be useful to say so. An exhaustive classification of a language into discrete varieties may well be a chimera, and attempts to produce one may one day be viewed as little more than the manifestation of

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a stylistic psychosis.[1]

To clarify the argument at this point, let us eliminate from the discussion those cases of stylistic uncertainty just referred to, and concentrate on the apparently very clear instances of situationally-distinctive features. The question which now has to be asked is, How do we verify our intuitions about the status of these features? Before we commence the quantitative part of the exercise, how do we know what to count? Do we simply 'notice' a feature, and assume that our allocation of it to a particular category is valid because we are stylisticians? This is scientific arrogance. It is true that previous linguistic training and experience of stylistic analysis may give us a sharpened intuition about what to look out for, but if this is all that is going on, then our position is really no different from that of the skilled literary critic. Intuition is no substitute for explicit criteria in this matter. Moreover, there is the point that the more stylistic analysis we do, the worse at stylistic analysis we may tend to get. It is a commonplace that people who have worked on surveys of English usage, and the like, are often very bad at giving off-the-cuff opinions about usage, as their intuitions are too flexible. Being at the opposite end of the pole from traditional prescriptivism, they will accept as permissible English far more than the 'average educated native speaker' will. And the same goes for stylistics. My own error is not to miss something out altogether in analysing a text, but to read far more in than the text might reasonably bear. A similar point is often made about editors of literary texts. So, how do we determine the validity of our intuitions? This is the really interesting question, but it has not, as far as I know, been faced. I am aware of no acceptability test (cf. Quirk & Svartvik, 1966) for stylistic data, using stylistically-naive native speakers as judges; nor do I know of any analysis of the variability in stylisticians' reactions to data. I shall discuss both these points in turn. In effect, what I am asking for is a stylistic analysis of stylistic metalanguage.

Perhaps one reason for the lack of development of any validation procedures here is that the real complexity of the problem has not been appreciated. One aspect of this complexity, which is relevant for the discussion of both intuitions about stylistic features and intuitions about the typicality of texts in a given variety (see above), is due to the existence of linguistic stereotypes. A stereotype is an individual or group's conventionally held, oversimplified mental picture of some aspect of reality: it corresponds in some respects to the reality of an event, but exaggerates, distorts, or ignores others (see Crystal, 1971b, for the application of this notion to concepts in phonetics). For instance, if I tried to speak in legal English (as in a joke), then I would introduce certain features that I felt were characteristic of lawyers speaking or writing (e.g. 'notwithstanding', 'hereinbefore', 'the aforesaid gentleman'), and this would probably be enough to get my reference recognised as such, though it would

[1] The claim for exhaustiveness, in relation to language teaching, is made for example by Corosch (1970:4, ff), one of whose objectives is 'complete typological inventory of language situations'. For the opposite viewpoint, see Wilkins, 1971: § 3.2, 'Even where we know the general purpose for which a learner is acquiring language (the macro-situation), it does not follow that this overall aim can be segmented into smaller situational units, each of which is in turn behaviourally defined.'

certainly at times be little more than a poor parody. (Cf. Quirk 1961, where there is some discussion of conventional representation of dialect pronunciations in orthoggraphy.) Or, to take a different example, one does not have to be a believer to appreciate something of the force of a satire using religious language : educated atheists are just as able to identify and assess the overall effect of at least some features of religious discourse as anyone else, even though these may not be the central ones, from a stylistician's point of view. A good example would be the use of archaic language, which is probably the number one feature of a stereotyped view of religious discourse, though such structures are nowadays almost totally absent from liturgical, biblical, etc. language. And, as a third instance of a stereotype, there is the view of business English as containing many formulae (of the type 'Further to yours of the 11th ult'), a kind of language which these days most businessmen and business manuals try to avoid.

What theoretical status have these stereotypes? Should they be given any recognition in our stylistic models? It seems to me that explicit recognition of the concept of stereotype is an essential step for stylistics to take. It is important because it accounts for the existence of two stylistic intuitions, or 'modes of knowing', on the part of the native speaker, which should not be confused (I exclude for the moment the complications introduced by the possession of a third intuition, in the case of a linguist). Situationally-distinctive features constituting a hypothetical variety may be recognised in either of two ways, depending on whether one is involved in the variety 'professionally', so to speak, or not. As a lawyer, I will have a view of legal language, an awareness of the reasons for the form it takes (e.g. why much of its written medium is punctuationless, why lexical formulae such as 'without let or hindrance' are used), which a legally naive native speaker will not have. But, as a legally naive speaker, as I have suggested, I will have *some* ideas about what goes on, even if this is only from films, television, novels, and the like. Is my stereotyped view of any relevance to the stylistician? I argued above that a stylistic analysis had to be as compatible as possible with the 'professional' mode of knowing (in discussing the selection of television advertising); thus, when Davy and I were writing the chapter on legal English in *Investigating English Style,* we took pains to read up on manuals of legal expression, and to have our text and our analysis commented upon by legal colleagues. But it does not follow that, because we considered analysis of the professional mode a priority, we should not wish to pay attention to the 'lay' mode. On the contrary, I have some sympathy for those who might argue that *the* important phenomenon for stylistics to account for is the intuition of the lay language user on these matters, and I certainly think it should be studied.

This issue reminds me in some respects of the question posed by theory of literature as to whether the valid meaning of a text is that which corresponds to the author's intention, or whether a variety of individual readers' interpretations are equally valid. And the arguments which are familiar in that debate apply here too, in particular the point that as we shall never achieve a *full* understanding of legal language without becoming a lawyer, therefore the notion of a complete stylistic analysis of the professional mode becomes irrele-

vant for most practical purposes. The important question, for, say, the teacher, is How much of this complete analysis will the student *need* to know? It is this question which a field which might one day be called 'applied stylistics' might profitably begin to investigate. Meanwhile, what contemporary stylistic theory has to do is consider precisely what status the data it is supposed to be accounting for has. I am often confused in reading articles on stylistics as to whether a piece of illustration represents the intuitions of the professional native speaker, the lay native speaker, or perhaps someone else. It is conceivable that if the concept of stereotype is accepted, it will do much to clarify ambiguities in analysis of this kind. It provides an intermediate theoretical position which on the one hand avoids the totally introspective approach to analysis (which stylistics developed largely in reaction against), and on the other hand avoids the too powerful constraint that all shared reactions to stylistic features ought to be identical with those specified by a complete, 'professional' stylistic analysis. However, I do not know how to begin investigating stereotypes : it is a complex psycholinguistic concept which will doubtless require fresh techniques of analysis, including some new thinking on validation studies. And until I know, I do not feel I can safely and confidently make recommendations about usage to enquirers, such as in the field of foreign language teaching.

Some kind of test which would establish the generalisability of my stylistic intuitions is very much needed, then, as a *routine* research tool. I am not concerned only about the cases where two stylisticians are in open disagreement, where such a test would clearly be useful. Such cases are not common, in my experience. Far more frequent, and more worrying, are the cases where two stylisticians do not know they are in disagreement, because they are using the same category labels for a stylistic effect, but giving them different senses. What do labels like 'legal', 'formal', 'upper-class' and so on actually mean? I do not know, but one thing I do know is that they do not mean the same things to all men. A critical analysis of descriptive labels which displayed considerable disparity behind a commonly used terminology has already been carried out in the field of intonation studies (see Crystal, 1969 : Ch. 7); and a similar kind of divergence is emerging when one analyses the way in which native speakers apply stylistic labels to pieces of text. In a project investigating the use of the labels 'formal' and 'informal' in English, for instance, Chan (1969) has shown that there is considerable disagreement between native speakers and inconsistency within individuals as to how these labels should be used. What is formal for one person may be informal for another; and the more intermediate grades of formality one recognises, the worse the confusion gets. Such terms as these are by no means self-evident, and should be carefully watched. There may be no common-core of usage which accounts for our ability to polarise texts in terms of a single formality scale. After all, to say that a sermon is 'formal' is by no means the same as saying that an election speech is formal, as the latter has a greater possibility of becoming informal than the former.

This problem is not solely a terminological one, however. If we allow the distinction between competence and performance to be introduced into the argument at this point, then it would surely be claimed — at least by those

who recognise a more flexible kind of competence than Chomsky apparently does (e.g. Lyons, 1971) — that at some stage we have to investigate stylistic competence, in some sense. That is, we are not interested in investigating solely a lawyer's (say) reaction to a feature we propose to describe, but also his view as to how typical this feature is, either in his idiolect, or in the variety as a whole which he professionally uses. If we find in a text four adjectives before a noun, for instance, then what should our stylistic statement be? Presumably none of us would want to say 'In this kind of English, a distinctive feature is that four adjectives may be used before the noun', and stop there. Stylisticians do not in fact say this kind of thing very much. What they tend to say is 'In this kind of English there is complex premodification using adjectives', or 'There is the possibility of long sequences of adjectives being used'. Notions of length or complexity are of course only as meaningful as the amount of data which has been analysed comparatively. In the present state of stylistics, such notions *can* be used, it seems to me, because very little data has been analysed. In our book, for instance, we frequently make use of such notions, but we always try to make their application clear by referring any descriptive statements about length or complexity to the sample of conversational English which we chose as a norm (see Crystal & Davy, 1969 : 95), and we try to keep the comparative part of our analysis within the scope of the samples in the book. As more and more data gets analysed, though, this situation cannot continue, and theoretically valid measures of complexity, and the like, must be found if stylistic analyses are to continue to be meaningful and consistent. Meanwhile, I think it is important for us to recognise that the intuitive leap which we make between the statements 'Four adjectives may be used . . .' and 'Long sequences of adjectives may be used . . .' is completely unscientific without the basis of our judgement being made quite explicit.

The third place at which evaluative notions seem to be unavoidable in stylistic analysis is at the very end, in what we might refer to as the 'renewal of connexion' between our stylistician's persona and our persona as ordinary language user. Once we have satisfactorily (sic) established a set of stylistic features, and counted them, and drawn up a comparative account of their occurrence and distribution among the texts of our sample, then what? Is there always a non-arbitrary, objective way of deciding whether two texts (or sets of texts) can be considered samples of the same variety? In most stylistic research, the assumption has been that statistical techniques will be adequate to this task, and the illustrations of varieties generally given are usually of such distinct kinds of English that one might be forgiven for thinking that demarcation lines are invariably clear. In fact, statistical analysis rarely gives a clear answer, in my experience, and requires reference to qualitative criteria at a number of points (cf. Reed, 1949 : 235,ff.). There is, for instance, the decision that has to be made as to which statistical measures are likely to be the most appropriate to handle a problem — let alone the question of whether *any* normal statistical techniques are really appropriate for the kind of problems presented by language samples of this kind. A typical stylistic analysis of two texts will display varying degrees of identity and divergence throughout all levels and ranks of linguistic structure (perhaps I should say, 'in principle', as few stylistic

analyses ever approach comprehensiveness in this respect — but cf. Moerk, 1970). Using Halliday's terminology, we can readily imagine a situation where two texts are almost identical at sentence rank, less so at clause rank, very different at group rank, identical graphologically and lexically, slightly different semantically, and so on. A single statistical assessment of structural identity is meaningless in such cases, for obviously from a given statistic one would be able to say little about the underlying configuration of structure which gave rise to it. And this situation is typical. Thus, at some point in our study, we have to decide on the degree of abstraction at which a quantitative analysis might be usefully made (at what level of delicacy, so to say), and make some kind of statement about relative importance of variation at the different structural levels. Immediately, the question becomes one of evaluation, and the usual, largely subjective criteria of elegance, simplicity, and so on, are raised.

But even assuming that arbitrary decisions have been made on these counts, there remains the general question of assessing the 'amount' of statistical difference and similarity between samples of an assumed population. If we have collected ten samples of journalism, let us say, and wish to establish that this label is stylistically meaningful, then we have to establish that the differences between the samples are insignificant. Unfortunately, language being the way it is, the application of most statistical criteria, such as the x^2 test, shows that most differences are significant, though some differences are vastly more significant than others (x^2 results up in the hundreds are by no means uncommon, even for such 'stable' varieties as scientific English — see Thakur, 1968). Of the ten samples, for instance, two might be so different that this might justify a decision to sub-classify the label journalism — say, into 'popular' v. 'educated' press report; but the others might be spread between these two, in such a way that there is no clear boundary-line as to where these two sub-classes of journalism part company. Unless then, we wish to argue that each sample is its own variety, we are forced to make some kind of intuitive grouping, on situational grounds: there may be no greater statistical difference between samples 5 and 6 as between 6 and 7, for instance, but we will choose one and not the other on intuitive grounds (that it produces the 'best' analysis), e.g. by convincing ourselves that the *Guardian* is educated whereas the *Telegraph* is not. But such an analysis is circular, and makes any descriptive stylistic statements vacuous. Without a much more refined statistical and data analysis, and a more sophisticated linguistic theoretical notion of evaluation procedures, I do not see how this circularity can be avoided. Meanwhile, the difficulties should at least be recognised.

So far I have been arguing that many of the assumptions underlying stylistic theory and method need to be made explicit and tested in some way; otherwise our stylistic analyses will become naive simplifications, capable of being shot down by the first sharp teacher who reads our findings. There have, in short, been too many attempts to produce taxonomies of stylistic effect, with too little attention being paid to the criteria which should form the basis of the taxonomy (or indeed, to the more fundamental question of whether varieties should be studied taxonomically at all). As a result, theoretical terms tend to multiply redundantly or be used inconsistently. On their own, terms like

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'register', 'tenor', 'field' or 'situation' seem innocuous enough; but when one tries to piece them together to make a complete theoretical picture, then one recognises the inherent weaknesses in many of the definitions. A term like 'register', because of its breadth of definition, is almost bound to produce confusion. Any situationally-distinctive use of language may be called a register, it seems, regardless of what the most important criteria of distinctiveness are. Newspaper headlines, church services, sports commentaries, popular songs, advertising, and football, inter alia are all referred to as registers in Halliday, McIntosh & Strevens, 1964 (pp. 88-9). The danger, of course, is that people new to this field will think that they will be saying something new by referring to these uses of language using the term register, and that because these uses can all be labelled in the same way, that they are therefore the same. But they are not the same: different situational variables are involved in each case. For example, 'sports commentary' conflates two distinct notions, that of 'sports reporting' and that of 'commentary form'; 'football' is vague, but presumably this is an occupational notion only; and 'church services' could mean many things — would it include 'sermons', for instance, or is this a separate variety? This last point is a characteristic problem raised by the present approach. What level of abstraction produces the optimum characterisation of a variety? Is there *a* variety (or register, or whatever) of 'advertising', or are there *many* distinct varieties of advertising (e.g. newspaper, television, public announcements), or are these best regarded as 'sub-varieties'? Is there, in turn, a sense in which advertising may be viewed more abstractly as a 'sub-variety' of, say, propaganda? Without very explicit criteria, there is no way of avoiding inconsistent judgements on different occasions, e.g. viewing the different categories of advertising as different varieties, but ignoring the quite comparable differences which mark the various categories of scientific language (e.g. reports, laws, definitions, experimental instructions). This difficulty gets worse the more languages one studies. So far, stylistics has been very Indo-European in its orientation. It is difficult to see how it will cope with some of the situational categories developed by anthropologists, for instance, to talk about the variations they have noticed (Crystal, 1971c). The 'choice' factor already referred to is an example of a criterion which seems much less relevant when one discusses bargaining dialogue between tribes, and the notion of restricted language (which on the whole receives little mention in stylistics) seems much more relevant. Difficulties of this kind will disappear only if we develop a thorough understanding of the basis and limitations of our terminology, and perhaps a comprehensive survey will not be long in forthcoming. It is certainly much-needed, for while I have heard it said that the terminological disagreement is a healthy sign of a developing subject, myself I prefer to see it, less optimistically, as an inevitable outcome of confused thinking.

I have spoken so critically about a branch of linguistics which is generally uncriticised, because I feel that one of the jobs a conference of the present kind can usefully do is make the people who formulate policies and write textbooks aware of the difficulties as well as the facilities which come from a contact with our subject. In this way, I trust that attention will be paid where it is needed. I am not myself too pessimistic, however, regarding the future

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relationship of stylistics to foreign language teaching, if a careful analysis of the requirements of the language teacher and learner takes place, and over-ambitious projects (such as large-scale variety analysis of a foreign language, cf. Gorosch, 1970 : 6) are avoided. What we need, of course, is a job-analysis of our own job. What exactly are the foreign language teacher/learner's stylistic needs? Can they be categorised, and will these categories correspond to the theoretical constructs already recognised in the stylistics literature? I do not know. There seems to have been a fair amount of discussion about applying a given set of stylistic categories to foreign language teaching situations, on the assumption that they will be relevant, but little study of what happens when the reverse approach is made. Let us examine the implications of this a little.

There seem to be a number of reasons given arguing for the relevance of stylistics to foreign language teaching. Firstly, it is hoped that an awareness of stylistic variation will provide a rationale for selecting a particular variety to teach, and ensure that a single stylistic level is maintained consistently as the basis of a course. (In other words, stylistic awareness is *not* to be seen as solely the province of advanced language teachers, as has sometimes been suggested. Most stylistic effects, it is true, can be explained only by reference to the idea of choice between alternative constructions, which presupposes a certain minimum of structure to have been acquired by a student. But any selection of materials, even at introductory level, implies a stylistic selection, and this has to be made consistently, with the author as fully aware as possible of the consequences of his choices at all points.) Secondly, it is claimed that stylistic awareness will allow for a principled introduction and grading of categories of stylistic effect different from the variety which has been chosen as a norm, and thus promote a more systematic coverage of the 'resources' of the language than would otherwise be possible. Thirdly, stylistics brings with it methods for dealing with the analysis of any specific difficulties involving situational variability in speech or writing. In so doing, it will provide a terminology for describing stylistic effects, and a means of relating these to the 'common-core' features of the language. Fourthly, stylistics accumulates facts about usage not otherwise available; ideally, a comprehensive 'dictionary' of stylistic 'meanings'.

Now stylistic analysis, at least in principle, seems able to satisfy all these requirements : each requirement clearly relates to a task which theoretical stylistics has already recognised as important and meaningful, and research carried on within stylistics (if done properly) thus looks as if it will be relevant.

We can now ask the question : to what extent can stylistic notions be incorporated within foreign language teaching procedures, as these are generally viewed at the present time? This is a vast question, so I propose to restrict it by illustrating just two of the theoretical problems which arise when one tries to turn this relationship from theory into practice — one in connection with error analysis, and one for theory of testing. Taking the case of stylistic errors first, it is generally recognised, both in mother-tongue and foreign language teaching, that a stylistic error is in principle different from a linguistic error per se. A linguistic error refers to a usage which could not occur in *any* context of English use; a stylistic error refers to a usage which is inappropriate in the situation in which it occurred, but which could have occurred in some

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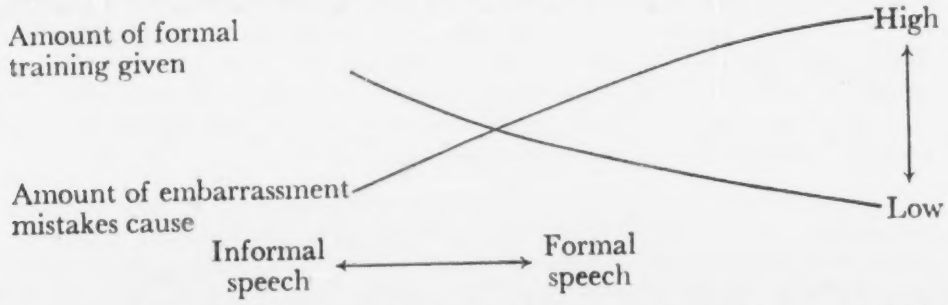
other situation. But there is of course more to it than this. Depending on the degree of restrictedness of usage of the feature, so there will be a gradation in the likelihood and seriousness of stylistic errors. As I mentioned earlier when discussing formality, some errors are more serious than others, because some situations are less permissive than others. In conversational English, for example, if foreigners make mistakes, then from the stylistic point of view they are relatively unimportant, as conversation tolerates more 'noise' and is more flexible than other varieties of English. Mistakes are missed, ignored, or joked about. On formal occasions, however, where it is more important to 'make a good impression', stylistic mistakes are going to be more serious. Introducing a stylistic perspective into foreign language teaching thus brings with it a certain tension : on the one hand, under the influence of linguistics, language teaching has begun to recognise the centrality and distinctiveness of everyday conversational English, the more formal kind of English in the older textbooks being considered artificial; on the other hand, it is the more formal kinds of English which present the greatest problems as far as social acceptability is concerned. At the moment, the pendulum seems to be swinging well into the conversational end of any formality scale — which is alright, so long as the more formal varieties of spoken English do not thereby become ignored. If there was nothing better to do, one might spend some time developing a scale of linguistic embarrassment, which would reflect this state of affairs, e.g.



This may not in itself be a particularly serious pastime; but it does I think indicate the kind of issue raised when one tries to incorporate stylistic reasoning into one's general practice.

If we turn now to the relationship between stylistics and testing, we find a different kind of problem posed. It is, to begin with, difficult to see how stylistic awareness fits in with some of the evaluation procedures language teachers refer to. This point is clearly illustrated if I take the six criteria postulated for the evaluation of oral and written proficiency in modern languages at a Council of Europe conference on 'Continuous assessment in upper secondary education' (held at Sundsvall, Sweden, in July 1969). These criteria are : pronunciation and accent (sic), grammar and structure (sic), vocabulary and idiom, fluency, comprehension, and subject-matter. Stylistic awareness would seem to be a separate dimension, relevant to all these areas, but not easily subsumable under any one. Fluency would seem to be the nearest relevant category, but this is not particularly satisfactory. It is rated in the above procedure on a 7-point ascending scale, as follows : 'negligible', 'disjointed and hesitant', 'uneven',

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'satisfactory though somewhat erratic', 'rarely hesitant', 'great facility', and 'comparable to the cultured native speaker'. Now it is not for me to try to assess this kind of approach to testing; but I would like to point out the difficulties which it poses for the stylistician. (I shall not discuss questions of linguistic criteria here, e.g. whether there is an overlap between the features referred to by these ratings and those on any of the other scales.) The main problem is caused by the highest point on the scale, which is a commonly cited theoretical terminus in foreign language teaching. What exactly does it mean, stylistically? It seems to me that there are two difficulties in the phrase, one involving the notion of 'comparability', the other the question of defining the stylistic normalcy of the 'cultured native speaker'. This last point is rarely raised in discussions of foreign language teaching: as long as the speech model one is using is not 'sub-standard', it is likely to be acceptable for foreign consumption. But in the context of mother-tongue teaching, the concept of a cultured native speaker, and the fluency which he is supposed to possess, is by no means clear (as recent discussion of oracy has shown). Not only is the educated native speaker supposed to achieve norms of fluent expression and comprehension which satisfy the basic functional requirements of communication, he is also supposed to develop standards of appropriateness or aesthetics which have been laid down by the community to which he belongs (or, at least, by some part of it). The job of the mother-tongue teacher is not simply to help the students to communicate, but to help them communicate efficiently and effectively. In other words, an evaluation procedure for stylistic awareness in foreign language teaching must ultimately be measured against a more general procedure designed for native speakers; and this, in turn, suggests the need for a much more integrated approach to the problems of language teaching on the part of L1 and L2 scholars than is at present available.

Turning now to the question of comparability of foreign learner to native speaker, we are faced with a number of problems. The most interesting, in a way, is to consider the implications of taking literally the idea of 'speaking a language as a native speaker does'. If we take conversational English as an example, this being the clearest case in point, then I wonder to what extent the language teacher who claims he wants to teach this realises what he would be letting himself in for if he did so in a stylistically accurate way, bearing in mind the characterisation of conversation as 'normal non-fluency' by Abercrombie, Quirk, and others. I am not referring here solely to the hesitations and interruptions which take place in conversation, or the general lack of planning and randomness of subject-matter, but rather to the absolutely predictable use of minor sentences, elisions, anacolutha, parentheses, and loose coordination which distinguishes its syntactic structure. In lexis too, I would point to the use of items with a characteristically vague sense (e.g. 'whatsisname'). This kind of thing, I imagine, is *not* explicitly taught in language classes, so what status, stylistically, has the kind of conversational English that *is* taught? And to what extent should a teacher let himself be influenced by it? Clearly here we have another example of a tension between stylistic and pedagogical aims; but it is one which has rarely been given any explicit mention, presumably because it is but recently that large-scale analysis of

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really spontaneous conversational English has been taking place.

A related point concerns the analytic problems which stylistic analysis sometimes raises, and which the teacher who is putting his students into contact with a wide range of speaking or writing styles would have to face. Sometimes available grammatical models simply cannot handle some of the structures which emerge. A particularly clear case concerns sentence identification, which is an extremely difficult problem in conversation, as can be seen from the following extracts (taken from Crystal & Davy, 1969 : 97,ff., but omitting all prosodic transcription apart from an indication of pause) :

(a) A you got a cold —
B no — just a bit sniffy 'cos I'm — I am cold and I'll be all right once I've warmed up
... ...

(b) B my arms were aching
A m
B and I though well I'll get it on Tuesday

Conversation is characterised by a large number of loosely coordinated clauses, the coordination being structurally ambiguous : it is an open question whether one takes these as sequences of sentences or as single compound sentences, particularly in view of the absence of any clear phonological indications of boundary marks. This situation is illustrated in extract (a). How many sentences are there here? Extract (b) illustrates the frequent use of monosyllabic interpolations, and the problem is, Does the interpolation force one to recognise B's second utterance as a fresh sentence or not? It is not difficult to find descriptive problems of this kind in other varieties too (commentaries and liturgical language provide two very good examples). Presented with such problems, in addition to the ones he has already, one might forgive the foreign language teacher who felt that stylistics was not the panacea it had been made out to be.

A further problem arises in connection with the way in which terms like 'fluency' are used. If fluency is interpreted as meaning 'productive efficiency in language use', as it usually is, then the question of stylistic relevance is going to depend on an assessment of the student's needs in the foreign language, stylistically. And here one might well argue that these are very few, particularly if one views the pedagogic situation in terms of 'language for special purposes', as has been argued in a previous CILT Conference, or as situationally organised syllabuses, (in the sense of Wilkins, 1971)[1]. He will need a conversational

[1] 'A situationally organized syllabus is one in which the first step is to identify types of language learner, where the typology is based on the purposes for which people are learning the foreign language. The second step involves a detailed behavioral or situational analysis of the anticipated language events in which the learner will participate. The third step is the description of the linguistic content of each of these situations, which in turn is the input to lesson-unit materials'. One should also note one of Wilkins's conclusions (§ 3.7): 'If we wish to base a language learning syllabus on the notion of 'language for special purposes', we shall have to do it in the knowledge that our understanding of the linguistic contents will, for a long time, have a subjective basis'.

variety, a more formal spoken variety, a fairly formal written style, and perhaps a professional style as well. Most foreign learners will never need to productively use legal, scientific, literary language, and so on. It will not, in a word, affect their fluency if they are never introduced to more than these three or four basic varieties. Now if this were so, a stylistician would indeed have difficulty arguing for the relevance of the whole of his subject to language teaching. But there is much more to it than this, as one can see if the notion of 'fluency', on which I am hanging this discussion, is broadened to take account of (for want of a better term) 'receptive' fluency. By this I mean native-speaker-like awareness of (or sensitivity to) the full range of vocal (or graphic) stylistic effects in the language *of others*. (There seems to be no single term which maintains a balance between productive and receptive fluency, though 'command' gets near to it. The traditional notion of 'comprehension' is too restricted for the purpose, usually referring solely to the awareness of cognitive content, as mediated by syntax and vocabulary.) In the field of receptive fluency, the foreigner is on very similar ground to the native speaker : in principle he might be exposed to precisely the same range of stylistic effects, and find himself faced with precisely the same problems of interpretation. And in this case, given a descriptive framework incorporating all the stylistic features of a language, it is not difficult to see ways of introducing these features to a foreign learner, and evaluating his progress. One might, for example, present utterances, systematically varying one situational component and displaying the corresponding variation in linguistic form — a procedure which is commonly used in mother-tongue teaching, and which is, in effect, a stylistic substitution drill. Its value and practicability, of course, depend on the adequacy of the descriptive framework used as a basis. It would have to involve at least the eleven variables outlined below, and there are probably others. Maximum receptive fluency would involve building up the foreigner's ability to understand the full range of meaning and nuance presented by each of the categories listed here (my inventory could of course be considerably extended). For a full discussion of each of the main variables, see Crystal & Davy, 1969 : Ch.3.

1. *Individuality*, e.g. differences between male, female, child, homosexual speech or writing.
2. *Regional dialect*, e.g. American/British/Cockney English, foreigners' speech.
3. *Class dialect*, e.g. uneducated, upper class, public school English.
4. *Historical dialect*, e.g. archaic forms, old or young speech.
5. *Medium*, e.g. speech on the telephone, public address, handwriting, reading aloud, reading from notes.
6. *Participation*, e.g. monologue, dialogue, 'multilogue'.
7. *Province*, e.g. religous, legal, advertising.
8. *Status*, e.g. formal, informal, types of phatic communion.
9. *Modality*, e.g. commentary, telegrams, lecturing, letter-writing.
10. *Singularity*, e.g. literary identities, recognisable contemporaries (e.g. Queen, TV characters).
11. *Others*, e.g. baby-talk.

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Each of these examples could appear in either of the two modes discussed above, 'real' or 'stereotyped', hence this would be an extra dimension to the classification. For example, apart from recognising and classifying cricket commentary, one has to note its stereotyped association with West country accents (a distinction that seems to be shared by agricultural discussants on the BBC and Long John Silver, amongst others). Or again, the framework has got to allow for the stereotyped fact that clergymen, lawyers and undertakers speak monotonously. A further point is that each of the above categories has to be seen in different contexts of 'noise', reflecting as far as possible the actual constraints on receptive fluency affecting native speakers, where hesitations, interruptions and background noise in general presents itself in varying proportions and intensities.

Finally, in view of the popular use of the term 'fluency' in language teaching contexts, it is surprising that very little attempt has been made to determine exactly what is involved. How does one account for a reaction of 'fluent' or 'non-fluent' in the first place? How might one validate experimentally the categories of fluency referred to above? Well, one way might be to present a piece of language to judges, systematically varying certain features of it, and noting variations in terms of fluency (or some synonym). I have tried this informally, and on the basic of this it seems to me that a great deal more is involved in the notion than is generally recognised. It is not by any means reducible to a question of hesitancy, or the like. A small set of syntactic features are involved — in particular, the inter-sentence connecting devices (such as introductory adverbials). Omitting these causes severe disruption of fluency. But more important than this are the prosodic features of connected speech. Apart from the uncontrolled use of hesitation and tempo contrasts ('uncontrolled' is an important qualification here — controlled hesitation is highly effective in some speaking styles), these perhaps being obvious factors influencing judgements of fluency, there is the avoidance of pitch-range, loudness and rhythmicality variation, and the over-use of single intonation contours. It is surprising just how much common linguistic variation gets allowed in as factors affecting fluency judgements. 'Fluency' thus seems to be another one of those labels in need of evaluation. Is 'monotony' a feature of fluency? For some judges, it is; for others, it is not. Intonation, it seems, is of primary importance here. It is interesting, in this connection, that if we speak English as the intonation handbooks would apparently have us do, by producing sequences of tone-units in an additive kind of way, the result is by no means fluent. There seems to be no attempt to read into non-segmental phonology one of the most elementary principles of segmental phonology, namely, that when phonological units are juxtaposed, they modify each other. Tone-units modify each other, too, and form clusters — 'major' and 'minor' tone-units, for instance, as Trim pointed out years ago (1959) — and it is *these* combinations, or rather, a knowledge of their combinatorial properties, which seem to be the important thing in the analysis of fluent connected speech.

Problems, then, assail the stylistician from all sides. There are the theoretical problems which he has to resolve to put his own house in scientific order; and there the problems arising from the existence of a wide conceptual

and terminological gap between his academic motives and techniques and those of language teaching. Throughout this paper, I have insisted on the importance of much more data analysis than has so far been done, and on the need for the development of validation techniques for central assumptions. Only in these, rather unfashionable ways, it seems to me, will stylistics become the valuable tool of the language teacher that it is already being claimed to be.

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Commentary

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The analysis of the deficiencies of stylistics as a branch of the study of language which Crystal presents as the first part of his paper seems to me a brilliant formulation of just what the critics could justifiably say against the discipline as we know it. We are often told that when you have formulated a question sufficiently clearly you have answered it. My feeling here is that although Crystal has presented us with problems, he has done it so lucidly that it becomes possible to suggest how solutions worked out in related branches of study might be re-deployed to deal with the most substantial issues he raises.

I have only two things to say about the middle term of the title — *fluency*. First that its placing in central position seems to me a subtle device for indicating just how the wide-ranging first and third terms are to be taken — a device whose constraints Crystal has not wholly accepted; and secondly that I agree with everything he says on the subject.

Let me then turn to his analysis of the problem and procedures, real and pseudo, of stylistics. I don't think terminological issues are usually fruitful, but his statements on p. 34ff. seem to me to constitute a pseudo-terminological note, in the sense that they state a position on points of substance, not merely of terminology. I do not find it useful to use *stylistics* in a sense so broad that it subsumes all systematic variation within a language due to social group norms, and including, for instance, dialectal variation. I accept of course, that it is concerned with more than literary material, and that the notion of choice is probably relevant, but probably not criterial. If there is a case for a distinct branch of study called *stylistics,* the case, to my mind, rests on the distinction between texts (whether written or spoken) and systems. Stylistics, to me, is necessarily the analysis of language in use, a branch of performance study; whereas dialectology and various types of variety-study can operate as either competence or performance studies. Crystal's examples seem to show that he really takes a similar view; though he does apply the term *style* to *variety*. I am unable to follow in what sense the term is applicable. Again, it is true that we may not in the end be well-advised to operate with such distinctions

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as those implied in the terms *stylistics, sociolinguistics, dialectology* (p. 36), but Crystal does not really mean us to sink as many distinctions as he says in the stylistic deluge. It is still useful for him to be able to speak of *The Waste Land* as including various 'kinds of English, as well as bits of other languages' (p. 35). The distinction he makes here, between what characterises the text and what composes it, could not strictly be made if he applied his definition of *stylistics* rigorously. The bits of other languages would have to be treated as features of an English style and we would still have no means of distinguishing them as styles from the character of the whole. I would begin, then, with a hypothesis, that stylistics is the study of the use of language in texts; at the primary level in individual texts, though we would expect secondary levels to develop, leading us to speak of the style of say T. S. Eliot or journalistic reporting, etc. As an initial hypothesis I consider this to be justified by the widespread tendency of people to feel that there are distinctive uses which can be talked about. I would not feel this hypothesis was subject to direct testing, but that it could stand provisionally until we know enough about a) what any given language is in itself b) what other kinds of systematic variation operate in any given language to be able to formulate precise questions about what is distinctive in particular texts. I would therefore consider this, on a long-term basis, to be a hypothesis scientific by Popperian standards.

Because Crystal has placed stylistics firmly in the field of language variation I can, despite our differences, now turn with some confidence to the standards of scientific rigour which he proposes, and particularly to the criteria of objectivity and explicitness, by which he rightly claims that stylistics fails. He attacks, first, sampling procedures (p. 37ff.). His hypothetical solution is that the linguist should 'face up to the necessity of devising techniques for coping with evaluative criteria, and relating these to our own, more familiar, linguistic ones' (p. 39). But, as he continues, such techniques do not exist, and the alternative, of analysing all data within a field, is rarely practicable. Now, although this is a real problem of stylistics as practised, I think it is in theory and fact a pseudo-problem. For its existence depends on the demonstrably false notion that we have identified the 'fields'. The second evaluative problem, that of deciding which are the relevant stylistic effects (p. 39), will, I believe, evaporate in a similar way, for it depends on the also demonstrably false notion that we know what 'the varieties' are, and only need to characterise them (though some will stand out less boldly than others). The same attack will have *something* to say about those problems of evaluation relating to the stereotype (p. 40ff.), and what in my field of work are sometimes distinguished as in-group versus out-group diagnostics (p. 41ff.). The final evaluative problem, of deciding whether two texts are samples of the same variety (p. 43ff.) will, I think, already have been disposed of, and we may have the glimmerings of a way into the statistical problems (p. 43ff.).

Crystal makes much, in his Introduction, of his temerity. Clearly I am sticking my neck out a good deal further in making such claims as I have. What I think is at issue is that Crystal has stepped back from the practice of stylistics to take a good hard distanced look at it by what it claims as its standards, and when you stand beside him at that distance you realise for the

first time that the problems are just like those of a wholly different field. In fact, it is my belief that other work can throw fresh light, just as it has in turn borrowed light from yet other studies.

In the *Tyneside Linguistic Survey* we ask the question: 'How can we determine the ecology of varieties of spoken English in an urban area?' We want to know what varieties are used, by whom, quantitatively and qualitatively speaking, and with what change through time in varieties and distributions. So what is a *variety*? We have, like the stylistician, a provisional hypothesis that there *are* varieties, on the grounds, that native speakers speak as if they think there are ('He sounds like a Geordie, speaks like a book, etc.'). But we are acutely conscious that the varieties traditionally recognised (RP, Modified Regional, etc.) are crude, intuitive, overtly and covertly confusing. So we ask: in disciplines which have a longer and more rigorous tradition of distinguishing varieties, how is it done? We turn, of course, to the biological sciences, and we find (as other linguists have done before) that the best attack is provided by numerical taxonomy. The essentials of this method are that the number of criteria should be as high as possible, and that none of the criteria should be dependent on any other. How many is a lot is something we don't know exactly, but it seems obvious that the greater the number of variables scored the greater the independence from intuitive judgement will be. In the *TLS* we use some 300, many of which are ratios which can be broken down into many sub-variables. To apply this sort of analysis stylistically seems to me to call for a large concerted effort, after which the individual analyses so far attempted will look very different. But that one can, given the resources and the motivation, start on a sound footing by this means seems to be unquestionable. The outcome could be a series of profiles in multi-dimensional space, clustering more or less exhaustively into varieties; if they did not cluster, the hypothesis of varieties would have been disproved.

Having got out our objectively identified varieties, we want to do things with them — particularly to see how they are distributed in terms of frequency (how normal they are) and in terms of what kinds of people speak them. The relevance of the first doesn't call for comment. The question of social distribution does, for it has often been assumed, (e.g. by Labov) that the social frame is a simple one which can be intuited, namely the single scale of socioeconomic class. Our intuition, supported by the evidence of a pilot study, was that for the English situation this would not do. So we were, on the point of the social dispersion of speakers, at the same starting-point as with varieties: we did not know how or what to classify on. And we resorted to the same technique of numerical taxonomy: score for all the criteria you can think of and let the classification emerge. So we are looking at varieties existing in one multidimensional space, characterising a human population dispersed in another multidimensional social space. The mappings involved, by the way, are new to mathematics and computation; or so my colleagues tell me. This is a great advantage to scholars in the humanities wanting to enlist the aid of their computing colleagues. We are not just seeking their services, but pointing the way to original research in their field.

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It seems to me that exactly the same relationship exists between the characteristics of individual texts, the varieties postulated in stylistics and the distribution of them across multidimensional 'fields' — having the function of a commentary, and the subject sport, for instance (p. 45) — and that the stylistician's problem of defining relevant 'fields' is analogous to our problem of defining relevant social groups.

This leaves the problem of the stereotype. I would like to draw your attention to a most thought-provoking discussion of linguistic stereotypes in the Inaugural Lecture of T. E. Hope, Professor of Romance Philology at Leeds[1] It sent me back to think afresh about the role of the stereotype in variety identification and modification. Parody and hyper-correction, for instance, are often explained in terms of the stereotype. Labov's investigation of social stratification in New York English is based linguistically on the stereotype (he investigates only five phonological variables), just as socially it is based on one variable. The investigation works in the sense that significant correlations are identified. It may be the case that the stereotype is sufficient to account for New York variation, but it is certainly the case that no investigation has been made of anything else. We know what Labov has caught in his net, but we do not know what has slipped through it. One's intuitions are that although stereotypes operate in English English, there is a great deal they do not account for, and much experiment in applying some of Labov's questions to English informants confirms this impression. However, a secondary stage in the application of the principles of numerical taxonomy is the identification of diagnostics, i.e. a relatively short list of criterial identifying characteristics by which a variety can in future be identified once its existence has been established by the original long list of criteria. I won't go into the mathematics of how this is done, but I would compare the difference between the original profile and the diagnostic identification with the difference between a spectrogram and the stylised input to a speech synthesiser. As for a current test for the role and identification of stereotypes, I shall soon be doing an experiment on this as it relates to varieties. Possibly stylisticians could find something in these ideas to apply in their own field. The essential point is that stereotypes can be made to emerge from a sufficiently exhaustive taxonomic approach.

[1]Hope, T. E., Language and stereotype: Romance philologist's parable. *University of Leeds Review*, Oct. 1971.

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Describing the language learner's language

S. P. CORDER

Improvements in the methods and materials of second language teaching are likely to remain a matter of trial and error until we have a better understanding than we have at present of the processes of learning a second language. The amount of research in this field is small and the results disappointing. Too much has been piecemeal and too much operational and local in its validity. We need a more general and pure attack on the problem. The suggestion that has frequently been repeated, that language learning is some obscure and little understood process of data processing, is a potentially fruitful one likely to benefit from the now increasing amount of work done in the psychology of cognition. The analogy often proposed for language learning is couched in terms of computer terminology — data processing, input, output, operations and so on. On this analogy the data of the target language to which the learner is exposed represents the input; the learning process, the data processing operation, and the output a grammar of the target language. There are great dangers in this analogising. The fact is, of course, that we do not control the learner as we do a computer: we do not control the input, we do not control the operations performed on the data and we have only the sketchiest picture of what the output is. Furthermore the learner is not simply a data-processing machine, but also a learner. That is, the programme is constantly being modified in response to feedback from the processing operation itself. If we must use data-processing terminology we must make a number of distinctions in our terminology. In one sense the teacher controls the data. It is in his hands to present or withhold potential input. It is not in his hands to ensure input. He has a number of techniques whose object is to ensure input, but his only means of checking whether input (or intake) has taken place is by making inferences from the linguistic performance of the learner. Furthermore, he has a number of techniques whose object is to control the processing by the learner of the input.

The teacher has nothing strictly comparable to a programme which he can feed into the learner to determine the operations the learner must carry out on the data, although the syllabus is in one sense part of a programme.

In another sense the learner is pre-programmed to process the input in a particular way. It may well be that some of our teaching techniques interfere with the smooth running of the learner's programme. It may also well be that the sequence of data presentation, the syllabus, does not correspond with the logical processing requirements of the learner. That some data is presented prematurely so that it cannot form part of the intake or that it is not readily available when it is logically required.

Research into the process of learning would seem, therefore, to be most fruitful if we could discover as a first step the correlations between the nature of the data presented with the state of the learner's grammar. That is, of course, not the same as saying that we are concerned with relating *input* with *output*, since, as I have suggested, we do not control the input and we can only infer the nature of the output from the learner's linguistic performance.

We already control the nature of the data and its sequence of presentation (at least in a formal teaching situation). What we have at present is only a rather inadequate means for inferring the nature of the learner's grammar.

The system is a dynamic one. Since it is impossible to feed all the data into the learner at one time and leave him to process it, like digesting a heavy meal, there will be a theoretically infinite number of states of his grammar. Since we do not know the optimum sequence of feeding him the data, we need to make a regular series of checks on his grammar to see the effect that exposure to certain data has had on the state of his grammar. By this means we might eventually discover what the optimal logical sequencing of the data was. Alternatively, as I have suggested elsewhere we might allow the learner to seek his own data rather than impose some arbitrary sequence of presentation upon him. Whichever procedure we adopted, we still need a means for describing successive states of his grammar. The situation is, of course, similar to the investigation of child language acquisition. A difference being that in the case of child language acquisition we have even less idea of what the nature of the potential input is.

What we need then, I suggest, are longitudinal studies of a learner's language, a set of descriptions of his successive 'états de dialecte'. By comparing these and logging the changes and then correlating these with the data of potential input we can make inferences about the learning process. The problem with which I am therefore here concerned is that of making such descriptions and indicating the techniques available to us for doing them. These are, I suggest, of two sorts. What are called in the language of applied linguistics *testing* and *error analysis*. I shall consider each of these in turn.

The stated objectives of tests are several. Achievement tests are designed to determine what has been learned of a known syllabus, proficiency tests are intended to give a picture of the state of knowledge of a learner, unrelated to any particular course of teaching. Diagnostic tests have the more limited aim of identifying areas of the target language which are not yet mastered. Aptitude tests are, of course, rather different in as much as their objective is to

measure the potential ability of the learner to acquire a second language. What distinguishes the actual form of the three first sorts of tests is not so much the testing techniques as the contents of the test. It is a matter rather of *sampling*. Where achievement is being measured the sampling is based upon some known body of data — a syllabus; where proficiency is being measured, it is the whole language which is the body of data on which the sample is based. The difference between diagnostic and proficiency tests lies not in the data which forms the basis of the sample but in the use made of the results of the test. For diagnosis it is the features of the language which the learner shows, by his performance in the test, that he has difficulty with which are the focus of interest, whereas for a measurement of proficiency, it is just as important to note what the learner does know as what he doesn't know.

Proficiency tests then might provide a source of data for the description of a learner's 'état de dialecte'. They suffer, however, from two serious defects in this respect. Firstly the amount of data available from a test is very restricted. The test is based on a sample and this sample, since it is not related to a particular syllabus must sample the 'language as a whole'. One need say nothing of the problem of what might be meant by the 'language as whole' or on what principles a sample which is representative might be made. Clearly it will depend upon some theoretical model, either of language structure or language performance. Since the theory of sampling rests on the assumption that performance in a sample of tasks predicts the performance of all tasks, a proficiency test aims to give a quantitative measure of the learner's knowledge of the language as a whole and not a qualitative statement of the nature of his knowledge. It does not, therefore, provide the sort of data on which a description of the learner's 'état de dialecte' can be based. The second defect of proficiency tests is that they are constructed on the basis of the target language. They ask the question : does the learner know this or that category of the target language, can he perform this or that process in the target language? The questions are necessarily of a yes/no sort. Proficiency tests are not devised to ask the question : *what* does the learner know, what language does he use, what are the categories and systems with which he is working? To know that the learner cannot perform some target language operation, may be useful for teaching purposes but for the purposes of a description of 'état de dialecte' we wish to know what similar or equivalent operations he does use. It is in the nature of objective tests that the test items admit of only two solutions — right or wrong. It is possible sometimes to make inferences from the wrong answers as to the nature of the learner's language but that is not what the test is devised to reveal. This is not to say, however, that some experimental techniques might not be devised (and some have been) to reveal the sort of information we would seek.

There is here a whole new area of investigation into the learner's language by means of experimental or test methods still to be developed. Where tests can be regarded as an experimental approach to the study of the learner's language error analysis can be called the clinical approach to the same problem. Here it is not the experimenter who is determining the sample of data but the learner. But because the emphasis in 'error analysis' has till now, been almost

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wholly concerned with the practical objectives of planning remedial syllabuses and devising appropriate techniques of 'correction', it too has suffered from similar inadequacies as a technique for describing the learner's 'état de dialecte', as proficiency tests. It too has been predicated on the assumption that the learner is talking an inadequate version of the target language. It too has been target-language based and as the name of the technique 'error analysis' has implied, concentration has been on what the learner does not do right in terms of the grammar of the target language. The assumption being that the learner's grammatical and appropriate utterances are evidence that he is at least in part using the categories and systems of the target language correctly and appropriately.

So long as the objectives of 'error analysis' were strictly practical, in the sense I have suggested, there might have been some justification for these assumptions, although as I hope to show, I think they were mistaken. Anyone looking at the spontaneous utterances of a learner using his 'transitional dialect' for real communication purposes (by this I mean to exclude all exercises using language in the classroom) quickly realises that the superficial well-formedness of individual utterances in terms of the criteria of the target language is no assurance that error is absent. The utterances of a learner can be roughly classified into three categories : *superficially deviant; superficially well-informed and appropriate in the context;* and *superficially well-formed but inappropriate.* By the latter I mean that such utterances cannot receive the interpretation that a superficially equivalent utterance would receive if spoken by a native speaker in that context. To put it briefly, what we are concerned with in 'error analysis' is discovering the degree to which the learner expresses his 'messages' by means of the categories and rules which the native speaker of the target language uses. This means that that category of utterances which I have called *well-formed and appropriate* are of no interest (i.e. do not form part of the data of 'error analysis') because they are simply taken as 'utterances in the target language'.

If, however, our objectives in undertaking 'error analysis' are to make a description of the 'état de dialecte' of the learner then the 'well-formed and appropriate' utterances are clearly an important part of our data. But there is an additional reason for this, and it applies, in fact, just as forcibly to 'error analysis' for practical purposes. A learner's utterance may be both well formed and appropriate and yet erroneous; we can call such utterances 'right by chance'. There are two ways in which this can be understood. The utterance may have been learned as a holophrase, that is, learned as 'an idiom', when it is, in fact, in terms of the target language, generable by perfectly general rules. An example of this was provided by a learner who quite appropriately produced : *What are you doing tonight*? Examination of a greater body of his other utterances revealed that he nowhere else used the progressive form for future reference. And yet, of course, this sentence is in no sense idiomatic in English. Secondly grammatical and appropriate utterances may be produced by rules which are not those of the target language. There is plenty of evidence that this occurs. To give just one simple example : the learner of German who produced the correct noun phrases : *Die guten Bücher, meine besten*

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Freunde, diese jungen Leute also produced the deviant noun phrases: *viele anderen Frauen; wenige schlechten Fehler; einige ungewöhnlichen Sitten.* From this one inferred that the learner did not yet know the rules for the endings of adjectives in the prehead position (i.e. strong and weak declension). He worked on the principle that the adjective was always inflected with the ending *-en* when preceded by a determiner (in the nominative plural). His rules were *not* those of the target language, but produced a fair number of superficially correct and appropriate forms *by chance.*

One is, therefore, led to the conclusion that 'error analysis' necessarily involves as part of its data fully acceptable utterances. If this is indeed the case then the name 'error analysis' becomes somewhat misleading, since one is forced into the position of saying that *all* the learner's utterances are potentially erroneous whatever their surface structure, or appropriateness may be; or, and I prefer to express it this way, whatever the surface form or apparent appropriateness of a learner's utterances, none are utterances in the target language. In other words, he is not speaking the target language at any time, but a language of his own, a unique idiolect, which no doubt shares many features of the target language.

The consequence of this is that the term 'error analysis' is no longer useful since it is based on the assumption that *only* his superficially deviant and inappropriate utterances are utterances not in the target language. This is what I meant by saying the 'error analysis' has hitherto been 'target-language based'. Furthermore, it also means that the term 'error' is just as inappropriate when the object of the analysis is a practical one, as I have characterised it, as when it is the more theoretical one of describing the learner's 'état de dialecte'. The position which I am adopting here is clearly the same as that adopted in the study of child language acquisition. The whole corpus of the infant's or learner's output is relevant data for the description of his language systems at any point in his learning career. The well-formedness or appropriateness of his utterances in terms of the adult language is irrelevant.

We can now turn to the more intractable problem of describing the learner's 'état de dialecte'. We have seen that proficiency tests, as at present devised, are unlikely to provide the type of data on which such a description can be based, not only because of their fragmentary character, and their target-language based criteria, but because the status of the learner's responses as 'utterances in his dialect' is in doubt. They do not provide utterances in a situational context of the sort necessary for any descriptive work to begin. This is not to say that they are 'contextless' but that the context is not one of normal communication. The status of his responses as utterances in his dialect is impossible to establish.

The usual data for a language description are the acceptable utterances of a native speaker. We immediately run into problems here when faced with the contextualised utterances of a learner. Firstly, the learner is not a native speaker of his 'transitional' dialect, in the sense that it is his 'mother tongue'. There are, in fact, no *native* speakers of his dialect. We can disregard for the moment that there may be other learners whose educational history and native

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language may qualify them to be regarded as speakers of the same dialect. It is true, of course, that, *in this sense,* each individual speaks a unique idiolect of his native language. This fact is disregarded by linguists, since they do not normally need to characterise the speech of individuals, and need only work, for their purposes, with such abstractions as 'a language' or 'a dialect'. For the linguist's purposes the notion of 'acceptability' has sufficient content since it can be shown empirically that there are groups of people identifiable by other than linguistic criteria, who agree over a wide range of data about the grammaticality and appropriateness of sets of utterances in certain given contexts. But for the study of language learning, I have suggested that is is necessary to be able to characterise the language of individuals. In these circumstances the problem of acceptability takes on a new aspect. There is only one solution : that every utterance of the learner must be regarded as an acceptable utterance in his transitional dialect. This is, of course, empirically testable by requiring him to assent to his own acceptance of his utterance. (I am ignoring necessarily the sort of mistakes that any speaker may make which are classed generally under the category of slips of the tongue. A learner speaking his transitional dialect is presumably as liable to such performance failures as any other speaker, indeed, probably more liable.)

Thus we come to the conclusion that the concept of ungrammaticality or deviance is not applicable to the learner. Everything he utters is by definition a grammatical utterance in his dialect. We have thus no problem similar to that which the linguist faces when undertaking the description of 'a language' of determining what his data are. We have, of course, the purely practical problem of paucity of data on which to work. This paucity is occasioned by the relatively low output of the learner, by the fact that he is the sole informant and more particularly by the fact that his dialect is, we hope, unstable. These are all problems shared by those who study child language acquisition.

The linguistic description of a language is of the sentences of a language. We are thus, in describing the learner's 'état de dialecte', faced with the question of the relation between his utterances and the sentences of his dialect. Let us now consider this in the light of Lyons' (1971) three processes of *regularisation, standardisation* and *de-contextualisation.*

Regularisation is the process of re-structuring an utterance in order to eliminate the sort of results of the adventitious failures of performance already referred to under the heading of slips of the tongue. The problem here is a real one, and is related to the question of acceptability. Since the learner is the only informant, our ability to *regularise* his utterances is crucially dependent upon his co-operation. It is true that a whole class of performance failures and interferences can be recognised without reference to the formal features of the utterance, for example, coughs, sneezes, hesitations, stutterings and so on. But the recognition of transposition, wrong orderings, substitutions of segments, can only be made by the learner himself. It is a practical problem in description that most work is done on written data where the learner is not available for consultation and self correction. In these circumstances surface deviations cannot be confidently and unequivocally assigned to performance failure or to

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features of the transitional dialect. We have no recourse to the linguist's own intuitions, though it should be said that many teachers become quite passable performers in their learners' dialect.

Standardisation, the second stage of idealisation, is that of restructuring the speaker's utterances to remove the systematic variation between utterances from different individuals due to personal and socio-cultural factors. At least at this stage no problems arise since the learner is sole speaker of his dialect. From a practical point of view it may be desirable to characterise the 'language' of a group of learners. In such cases the process of standardisation or normalisation may be necessary. For the purposes here being considered this does not arise.

It is at the crucial third stage, that of *de-contextualisation* that the most severe difficulties are felt, precisely because the learner *is* the only informant. Our ability to de-contextualise his utterances depends almost wholly on our ability to interpret the speaker's message or intentions. The fact that the linguist in this case is not a speaker of the learner's dialect makes the situation comparable both to the problem of describing the language of an infant or some unknown language. There are, however, certain advantages we possess. We can have recourse to the learner's mother tongue to establish the speaker's meaning. In this respect our task is much easier than that confronting those attempting to establish the sentences of the infant learner, and secondly, the learner's dialect, we assume and certainly hope, bears some strong resemblance to the target language. Otherwise the problem of contextualisation follows the usual course of making inferences about the learner's rules derived from similar utterances produced under different contextual circumstances. This is no more than to say that we do not infer the nature of a learner's sentences on the basis of the surface structure of one particular utterance.

So long as the study of a learner's language is target-language based, however, there is always the tendency to normalise and decontextualise in the direction of the target language, that is, to relate the learner's utterance not to the sentence of his dialect, but to the equivalent sentences of the target language. Thus, to give just one purely hypothetical example, if, in reply to the question : *whose car are we going in*? the learner were to reply : *John, if he gets here in time,* the tendency would be to relate this to the equivalent target language sentence : *we are going in John's car if John gets here in time,* whereas a more extended study of the learner's dialect might well show that the learner's underlying sentence was : *We are going in the car of John, if John gets here in time. A* decision as to whether this is the most probable account will depend upon the evidence from other utterances of the learner, that is, whether he elsewhere expresses possession by means of the *possessive case* or by *of,* or by some other syntactic device.

I can now summarise my argument. In order to make progress in the methods and materials of teaching second languages we need to be able to relate the materials and procedures used by the teacher to changes in the knowledge of the learner. For this we need longitudinal studies of learners expressed in terms of sequential sets of description of their 'états de dialecte'.

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The data on which these descriptions may be based could be drawn from proficiency tests or from error analysis, respectively representing the experimental and clinical approaches. At the present time both these approaches are target-language based, in the sense that the test items are devised, and analysis of errors are made, in terms of the grammar of the target language. It is proposed that a description of the learner's 'état de dialecte' can be better achieved by a recognition that what he speaks is not an inadequate or incorrect form of the target language but a peculiar transitional idiolect, which should be approached in the same way as the language of an infant or some unknown language. Then concentration on his specifically ungrammatical or inappropriate utterances which is characteristic of what is called 'error analysis' will lead to a distorted picture of his grammar. In other words, the concepts of *error* and *acceptability* have as little utility in the study of the learner's language as they do in the study of the infant's.

APPENDIX

Conference participants

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Professor E. C. Cherry, *Imperial College of Science and Technology, University of London (Telecommunications)*
Dr. D. Crystal, *University of Reading (Linguistic Science)*
Dr. L. Farrer-Brown, *Chairman of Board of Governors of the Centre for Information on Language Teaching*

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Mr. R. Fawcus, *Guy's Hospital* (*Human communication*)
Miss M. Fell, *Joint Council of Language Associations*
Dr. A. Fourcin, *University College London* (*Experimental Phonetics*)
Mr. J. F. Galleymore
Professor A. C. Gimson, *University College London* (*Phonetics*)
Mr. B. D. Green, *Centre for Information on Language Teaching*
Professor W. Haas, *University of Manchester* (*General Linguistics*)
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Dr. P. N. Johnson-Laird, *University College London* (*Psychology*)
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